

After the Breakdown: Persistent Changes in Voting Modes and Electoral Participation*

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Abstract

Administrative failures can affect participation beyond the election in which they occur. I study the aftermath of Berlin’s 2021 electoral breakdown using court-documented exposure and harmonized precinct returns from 2016 to 2026. An event-study difference-in-differences design compares in-person, postal, and total turnout in exposed and clean-control areas. In-person turnout remains lower in court-linked precincts across subsequent elections, while postal turnout increases in 2024 and 2025. In the 2025 within-federal-election comparison, in-person turnout is 1.77 percentage points lower and postal turnout 1.25 points higher; the net total-turnout estimate is smaller and statistically imprecise. The negative in-person result is similar under alternative geographic construction and conservative inference, although documented exposure is not randomly assigned. The estimates describe persistent changes in local voting-mode composition following the breakdown and its legal aftermath; they do not identify individual switching or persistent perceived voting costs. Alternative voting channels may accommodate adjustment without restoring the earlier composition of participation.

Keywords: election administration; turnout; voting costs; postal voting; election failure; difference-in-differences

JEL classifications: D72; D73

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1 Introduction

The costs of voting influence electoral participation. Administrative frictions can discourage turnout by making registration, travel, or the act of casting a ballot more costly. Yet a temporary administrative failure may also affect subsequent elections, even after the initial disruption has been corrected. I examine whether an unexpected breakdown is followed by persistent changes in the composition and level of electoral participation.

The distinction between participation and voting-mode choice is central. Following an in-person voting disruption, citizens can return to polling-place voting, use postal voting, or abstain. An alternative voting channel may offset some of the participation loss, but postal voting requires advance planning and may involve adjustment costs. Persistent in-person turnout differences therefore need not imply equally persistent differences in total turnout. The relevant empirical distinction is between a change in the distribution of voting modes and a change in whether citizens participate.

Berlin’s September 2021 elections provide a setting to examine these margins. Concurrent federal, state, district, and referendum votes were affected by missing or incorrect ballots, queues, interruptions, and temporary closures. Proceedings before the Berlin Constitutional Court and the German Federal Constitutional Court produced geographically specific records of these failures ([Bundesverfassungsgericht, 2023](#); [Verfassungsgerichtshof des Landes Berlin, 2022](#)). I combine those records with official election returns from 2016 to 2026, harmonized to 2021 postal precincts. Event-study difference-in-differences models compare court-linked affected precincts with clean controls using precinct and election-by-district fixed effects. The estimates capture changes in local participation associated with court-documented exposure, not individual experiences of the disruption. The 2021 breakdown was followed by a full rerun of the state election and a partial federal rerun, allowing changes in participation to be traced beyond the immediate failure and its legal correction.

In-person turnout remains lower in affected precincts across subsequent elections. Relative to the 2019 European election, the estimated deficits are approximately 1.1 percentage points in the 2023 state election, 2.3 points in the 2024 European election, 2.0 points in the 2025 federal election, and 1.3 points in the 2026 state election. Postal turnout increases by about 1.2 percentage points in both 2024 and 2025, consistent with partial adjustment toward an alternative channel. Total-turnout estimates remain negative in several post-2021 elections, although the within-state-election comparison provides weaker evidence of an aggregate loss in 2026. The most consistent finding is persistently lower in-person participation; the extent of postal compensation varies by election. This distinction matters for interpreting the cost of failure: a lower share voting at polling places need not imply an equal increase in abstention.

The paper contributes to research on the costs of voting and the institutional determinants of turnout. In the canonical voting calculus, participation depends on the costs of casting a ballot (Downs, 1957; Riker & Ordeshook, 1968). Registration, identification requirements, polling-place distance and relocation, opening hours, waiting times, early voting, and postal-voting arrangements can all affect participation (Barber & Holbein, 2020; Braconnier, Dormagen, & Pons, 2017; Brady & McNulty, 2011; Burden, Canon, Mayer, & Moynihan, 2014; Cantoni, 2020; Cantoni & Pons, 2021; Cantoni, Pons, & Schafer, 2025; Clinton, Eubank, Fresh, & Shepherd, 2021; Dyck & Gimpel, 2005; Garmann, 2017; Gerber, Huber, & Hill, 2013; Herrnson, Hanmer, & Koh, 2019; Pettigrew, 2021; Potrafke & Roesel, 2020; Thompson, Wu, Yoder, & Hall, 2020; Tomkins et al., 2023). Whereas much of this evidence concerns prevailing voting conditions or planned institutional changes, Berlin permits an analysis of participation after an unexpected failure of existing arrangements.

The immediate turnout effects of Berlin’s 2021 breakdown are studied by Fischer and Flügel (2025). Prior research also finds that long waits can depress participation in later elections (Pettigrew, 2021). The contribution here is the joint analysis of persistent in-person responses, subsequent postal participation, and total turnout following a publicly documented administrative breakdown and its legal remedies. Alipour and Lindlacher (2025) show that polling-place reassignment can induce mode substitution, while Bagwe, Margitic, and Stashko (2026) find that postal voting partly offsets the effects of increased polling-place distance. Related evidence documents the consequences of polling-place accessibility and assignment (Clinton et al., 2021; Tomkins et al., 2023). This paper extends that literature to adjustment following an administrative breakdown rather than a planned reassignment or convenience reform.

A distinction between temporary access costs and a failure of an established electoral service is central to the research question. Polling-place distance, relocation, and opening-hours changes alter the conditions under which an election is administered; their consequences can persist when access arrangements themselves remain changed. The Berlin failure, by contrast, involved a particular election day and was followed by new elections under ordinary legal procedures. The relevant empirical outcome is therefore not merely the contemporaneous response to a higher cost, but the subsequent participation trajectory after the initiating disruption. The design cannot hold all later administrative conditions fixed, which motivates the additional polling-site checks below.

The postal margin sharpens the contribution relative to studies of delayed participation effects. A decline in polling-place turnout after a bad voting experience may represent continued abstention, a change in the preferred mode of voting, or a combination of both. The

Berlin records allow these margins to be estimated separately at a common geographic level. The 2025 federal-election comparison is especially informative: it shows negative in-person and positive postal estimates within the same electoral family, without the change in the voting-age threshold that complicates the 2026 state-election comparison. The corresponding net turnout estimate is less precise. This contrast distinguishes persistence in the *composition* of participation from persistence in total abstention.

The results also relate to persistence in electoral participation and the consequences of prior voting experiences (Fujiwara, Meng, & Vogl, 2016; Gerber, Green, & Shachar, 2003; Meredith et al., 2009). Institutional conditions can affect the stability of participation routines (Bechtel, Hangartner, & Schmid, 2018; Gaebler, Potrafke, & Roesel, 2020). A severe failure may alter expected polling-place costs, disrupt habits, or affect confidence in electoral administration (Alvarez, Cao, & Li, 2021; Alvarez, Hall, & Llewellyn, 2008; Atkeson & Saunders, 2007; Bowler, Brunell, Donovan, & Gronke, 2015; Marx, Pons, & Suri, 2021). Precinct returns cannot identify these channels separately or establish individual mode switching; they document the aggregate participation response. Persistence may reflect learned expectations, habit disruption, or a more general response to institutional reliability, and these mechanisms should not be conflated with the observed precinct-level estimates.

The negative in-person result remains after adding controls, adopting district-clustered inference, restricting the comparison sample, and analyzing the original polling-station-precinct geography. The disruption-family and polling-site evidence is less conclusive about the underlying behavioral mechanisms. Postal voting provides a potential adjustment channel, although the estimates do not establish complete compensation.

The rest of the paper proceeds as follows. Section 2 develops the framework linking administrative failure to voting-mode choice and subsequent participation. Section 3 describes Berlin’s 2021 electoral breakdown and the administrative records used to identify affected areas. Section 4 presents the data, treatment construction, and empirical strategy. Section 5 reports the turnout results, identification and robustness checks, and mechanism-supporting evidence. Section 6 concludes.

2 Voting Costs and Mode Choice

The standard voting calculus compares instrumental and civic benefits with the private cost of casting a ballot (Downs, 1957; Riker & Ordeshook, 1968). Administrative failures raise realized time costs and uncertainty for some citizens. With postal voting available, participation becomes a choice among three alternatives: in-person voting, postal voting,

and abstention. Let $m \in \{IP, P\}$ denote in-person and postal voting, respectively, with the outside option of abstaining normalized to zero. The return to each voting mode is

$$R_{imt} = p_{it}B_{it} + D_{it} - C_{imt}. \quad (1)$$

A citizen participates when at least one mode provides a positive return, choosing the higher-return mode when both do:

$$\text{Vote}_{it} = \mathbf{1}\{\max_m R_{imt} > 0\}, \quad m_{it}^* = \arg \max_m R_{imt}. \quad (2)$$

The common-benefit specification is deliberately parsimonious. Ballot validity, the expressive benefits of voting at a polling place, or confidence in a specific voting method could also vary by mode. The essential distinction is that a higher relative cost of in-person voting need not reduce participation if postal voting remains an acceptable and sufficiently inexpensive substitute.

An adverse experience can affect the expected cost of later in-person elections, even if objective polling-place conditions subsequently improve. Write

$$\hat{C}_{i,t+k}^{IP} = C_{i,t+k}^{IP,0} + \rho_k F_i, \quad k \geq 1, \quad (3)$$

where F_i denotes exposure to the failure and ρ_k the possible additional expected in-person cost at horizon k . The sign and persistence of ρ_k depend on beliefs, experience, and subsequent information; $\rho_k = 0$ describes complete recovery. The expression is an interpretive device, not an estimated updating equation. Individual perceptions, the initial experience of each voter, and the adjustment parameter are not observed in the precinct returns.

Two distinctions matter for the empirical outcomes. First, a relative increase in expected in-person cost predicts a lower incentive to use the polling place, while its effect on total participation depends on whether postal voting remains preferable to abstention. Second, the availability of postal voting does not make adjustment costless. Applications, deadlines, and advance planning can entail procedural costs (Menger & Stein, 2020; Stewart, 2010). Familiarity with the process may affect the speed at which voters adopt it, giving a possible reason for postal compensation to emerge only in later elections. This is a hypothesis about choice costs; the aggregate data do not establish that particular voters learned or switched modes.

The contrast with a general loss of confidence in elections is also relevant. A shock that raises perceived costs or reduces expected benefits across *both* modes would predict weaker compensation through postal voting than a shock specific to polling-place access. Research

on election administration and voter confidence provides reasons to take this alternative seriously ([Alvarez et al., 2021](#); [Atkeson & Saunders, 2007](#); [Bowler et al., 2015](#)), but the present design cannot separately estimate a general confidence channel.

The framework distinguishes three observable outcomes. Persistent negative in-person estimates can coexist with positive postal estimates and little change in total turnout; alternatively, postal compensation can be insufficient, leaving negative total-turnout differences. Full recovery would eliminate the mode-specific differences as well. The relative responses of access-cost and ballot-error exposure provide an additional, more tentative comparison because their administrative classification and affected samples differ. These are implications for local participation aggregates, not identified individual transitions.

3 Berlin’s 2021 Electoral Breakdown

On September 26, 2021, Berlin held federal, state, district, and referendum votes on the same day, alongside the Berlin Marathon and under pandemic-related restrictions. Election officials had to distribute multiple ballots and organize voting across many local polling locations. The administrative failures that followed were not a uniform citywide closure but a geographically uneven set of problems. Documented incidents included missing ballots, incorrect ballot materials, queues, temporary interruptions and closures, and voting after the statutory closing time ([Bundesverfassungsgericht, 2023](#); [Verfassungsgerichtshof des Landes Berlin, 2022](#)). They differ substantively: queues and interruptions can impose immediate time and uncertainty costs; missing ballots can delay or prevent the act of voting; incorrect ballot materials implicate the validity or correctness of the vote even when physical access is possible.

The breakdown generated two court proceedings that form the empirical record. The Berlin Constitutional Court annulled the state election, which was rerun in 2023. The German Federal Constitutional Court subsequently ordered a partial federal rerun, held in 2024, following its review of the federal election ([Bundesverfassungsgericht, 2023](#); [Verfassungsgerichtshof des Landes Berlin, 2022](#)). The 2024 partial rerun is not included as a regular citywide election in the event-study panel because it did not provide an equivalent vote for the same set of affected and unaffected precincts. Subsequent regular federal, European, and state elections instead provide common comparisons across the original 2021 treatment geography. The 2023 state rerun and later regular elections are analytically useful follow-ups, not separately identified effects of the legal remedies.

The court materials serve distinct measurement purposes. The federal court-linked record

determines the preferred binary exposure definition. The state record is more informative about the type and reported severity of polling-place disruption. The two sources overlap but are not identical: some areas appear in both records, some appear in the federal record alone, and others appear only in the state record. The primary specification excludes state-only units from its control group. This avoids classifying a documented problem in one record as an unaffected location simply because it is missing from another. State-record families and waiting/closure measures are used in supporting analyses with correspondingly different samples rather than as interchangeable definitions of the primary treatment.

Postal voting is an established participation channel in German elections. It can avoid the need to attend a polling place on election day but normally entails requesting and returning ballot documents in advance. The timing matters for causal interpretation. Many postal votes in September 2021 were cast before the failures encountered at in-person locations became observable, so a negative 2021 postal coefficient in a treatment subgroup cannot itself represent substitution in response to an election-day queue or closure. Post-2021 elections provide the more relevant evidence on adjustment across modes. This does not rule out general pre-election concerns or contemporaneous administrative problems affecting other channels.

The episode was also unusually visible. Berlin respondents in the German Longitudinal Election Study reported concerns about electoral fairness and correctness and elevated reports of having been prevented from voting ([German Longitudinal Election Study \(GLES\), 2023](#)). This is contextual evidence that the administrative failure was salient. Because the survey cannot be matched to the court-linked precinct exposure used here, it does not reveal whether personally affected voters subsequently changed their expectations or preferred voting mode .

The institutional setting therefore provides three elements for an empirical analysis: documented geographic variation in administrative problems, repeated official turnout outcomes by voting mode, and later elections after the original failure and its judicial review. It also creates an important limitation. Court-linked exposure refers to the location associated with a documented problem, not to a verified individual waiting experience. Precinct-level estimates may incorporate changes in local electorates, subsequent administrative decisions, and the public response to the failure.

4 Methodology

4.1 Data and Treatment Construction

The analysis combines official Berlin election returns from 2016 through 2026 with the federal and state court-linked records of the 2021 breakdown. The source panel covers nine elections and 1,507 units on the 2021 postal-precinct geography. The preferred comparison contains 294 treated precincts and 1,056 clean controls, producing a balanced 1,350-precinct, nine-election analysis sample (12,150 precinct-election observations). The remaining 157 precincts are state-only or otherwise outside the preferred clean comparison. Baseline exposure is fixed across election years. [Appendix Table A1](#) reports the treatment-source counts; [Appendix Table A2](#) summarizes the panel variables.

Two geographical reporting levels must be distinguished. Election-day ballots are recorded by polling-station precinct (*Urnenwahlbezirk*, UWB), whereas postal votes are reported by postal precinct (*Briefwahlbezirk*, BWB). Their assignment and spatial boundaries are not constant across elections. The target units for the primary panel are the 2021 BWB polygons. An election-specific source unit’s reported vote and registered-voter counts are distributed across intersecting target units using a common, fine-resolution population grid. Crosswalk weights are calculated separately for UWB-level in-person counts and BWB-level postal counts. This preserves the accounting distinction between voting modes before aggregating them to a common target geography.

For each target precinct and election, the allocated in-person and postal voter counts are divided by the correspondingly allocated official electorate count to construct percentage-point turnout rates. Total turnout equals the sum of in-person and postal turnout, so the estimated mode-specific coefficients add to the estimated total-turnout coefficient in the identical regression sample. The spatial weights are based on a 2022 grid of German citizens aged 18 and older. They allocate *where* the election-specific administrative counts are assigned, but they do not replace the official registered electorate as the turnout denominator. In particular, weights summing to one ensure that citywide counts are conserved without proving that the distribution across target precincts is measured without spatial interpolation error.

The extent of interpolation varies across elections. The 2026 source boundaries generate more target intersections than the relatively concentrated 2024–2025 crosswalks, although earlier election geographies also involve substantial many-to-many allocation. The measurement audit records source-weight sums, overlap counts, and 18 harmonized precinct-election observations with total turnout above 100 percent ([Appendix Table A3](#); [Appendix Table A4](#)).

Concentration-weighted and restricted-crosswalk checks are examined with the results. These are reasons to validate the geographic approximation rather than reasons to assume every allocated precinct rate is an exact observed administrative quantity.

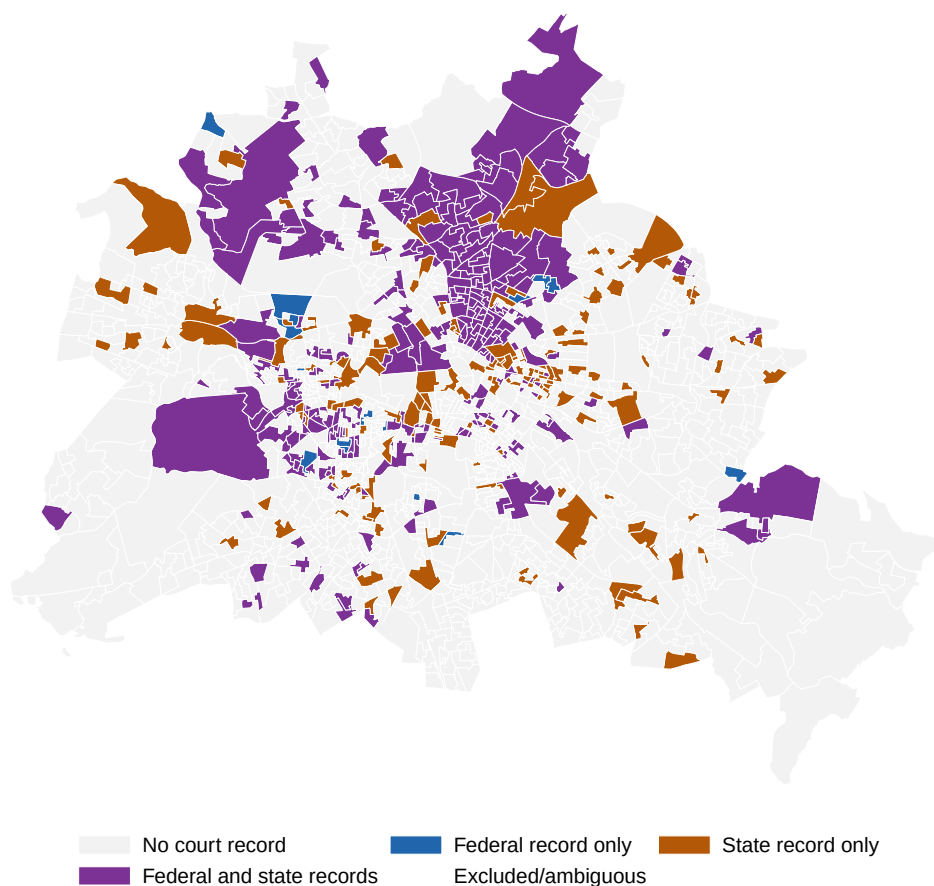
Electoral eligibility introduces a separate comparability issue. In the 2024 European Parliament election, the voting-age threshold was 16 and eligible non-German EU citizens could vote; Berlin’s 2026 state election also extended eligibility to German citizens aged 16 and older.¹ Official denominators reflect each election’s rules, but the fixed geographic population grid is not perfectly matched to these electorates. The resulting concern has two components: the participation behavior of the eligible population may change when the franchise expands, and the spatial distribution of newly eligible voters may differ from the grid used for allocation. A fully age-matched alternative crosswalk requires verified high-resolution counts and is not reported as completed in their absence. Within-federal and within-state comparisons help characterize election-family sensitivity but do not by themselves remove the 2026 franchise change.

The preferred binary treatment follows the federal court-linked 2021 polling-station precinct assignments. Clean controls are absent from both court-record sources, while precincts documented only in the state record are excluded. This definition is conservative about contamination in the comparison group, at the cost of excluding some areas and conditioning on administrative documentation. The corresponding geographic categories appear in [Figure 1](#). They should not be interpreted as a map of voters who personally experienced an interruption or as random assignment of administrative failures.

State records additionally distinguish waiting, voting after closing, interruptions, missing ballots, and ballot errors. The access-cost classification groups events plausibly impeding the ability to vote in person, while the ballot-error-only category contains documented errors without a recorded access-cost incident. A separate waiting/closure measure uses the maximum documented duration within an original polling-station precinct and aggregates it to BWB21 with eligible-voter weights. Federal-only observations lacking state-record severity data are not assigned zero severity. These analyses therefore have distinct measurement assumptions and smaller or differently composed treatment samples than the main court-linked specification.

¹For the 2024 European election, see <https://www.bundeswahlleiterin.de/europawahlen/2024/informationen-waehler.html>; for the 2026 Berlin election, see <https://www.berlin.de/wahlen/wahlen/berliner-wahlen-2026/fragen-und-antwortkatalog/artikel.1646712.php>.

Figure 1: Court-linked treatment exposure in Berlin



Notes: The figure maps 2021 postal precincts by source of court-documented election failure. State-only and overlapping state/federal exposure categories are shown separately because the federal and state records do not mention exactly the same precincts. Source-overlap counts are reported in [Appendix Table A1](#).

4.2 Empirical Strategy

The preferred specification compares the evolution of turnout in court-linked affected and clean-control precincts while allowing districts to experience different election-specific shocks. For outcome Y_{it} in 2021 postal precinct i and election t , I estimate

$$Y_{it} = \alpha_i + \lambda_{d(i)t} + \sum_{e \neq 2019} \beta_e (\text{Treated}_i \times \mathbf{1}\{t = e\}) + \varepsilon_{it}. \quad (4)$$

The three dependent variables are in-person, postal, and total turnout measured as percentage points of registered voters. Precinct effects α_i absorb stable local differences, including persistent differences in average political participation. Election-by-district effects $\lambda_{d(i)t}$ absorb common changes within each Berlin district at each election. Coefficients β_e measure changes in the treated–control turnout difference relative to the 2019 European election, conditional on these fixed effects. The main standard errors are clustered by 2021 postal precinct. Full baseline coefficients appear in [Appendix Table B1](#); the implications of district-level inference are assessed below.

The estimand is a location-level response to documented exposure, not the average effect of an individually verified queue or ballot shortage. The analysis fixes treatment according to the 2021 court records, while later electorates may contain different individuals. Accordingly, differences in mode-specific turnout describe aggregate changes in the use of voting channels, not observed individual transitions between voting in person, voting by post, and abstaining. The treatment also includes potential reactions to publicity and legal proceedings following the breakdown. Neither a specific expectation-updating parameter nor the effect of the judicial remedies can be separately recovered from this design.

The identifying assumption is that, absent the breakdown and its associated aftermath, treated and clean-control precincts would have followed comparable turnout changes within the same district and election. This is demanding because administrative failures and their documentation need not be random. Unadjusted treated–control turnout gaps are displayed in [Appendix Figure B1](#). The 2019 balance statistics show that exposed precincts are larger on average (about 1,884 residents versus 1,642), with differences in age and socioeconomic composition ([Appendix Table B2](#)). Fixed effects do not eliminate differential trends associated with these characteristics. Socioeconomic controls, matching, and comparison-group restrictions are therefore substantive checks rather than substitutes for the identifying assumption.

The two available non-reference pre-treatment elections, 2016 AGH and 2017 federal, provide a limited check. Their in-person estimates are small relative to the subsequent deficits;

the 2016 total-turnout coefficient, by contrast, is not negligible relative to the more modest later total effects. The pre-period elections also differ in election type, so failure to reject their coefficients does not establish parallel trends. Pre-treatment estimates, within-district placebo reassignment, and descriptive magnitude calibration are reported in [Appendix Table B3](#), [Appendix Figure B2](#), [Appendix Table B4](#), and [Appendix Table B5](#). The calibration is not a formal identified set for possible trend violations. District-clustered uncertainty is also reported ([Appendix Table B6](#)); precincts are nested within districts, so the two-way clustering calculation adds no separate independent clustering dimension.

Several additional analyses distinguish robustness of the main estimates from robustness of their interpretation. Within-election-type models assess whether combining different electoral contexts drives the pooled pattern; more concentrated crosswalk samples and original UWB geography test sensitivity to spatial allocation; alternative clustering and comparison samples address uncertainty and possible local contamination ([Appendix Section B](#)). Finally, the separate state-record models compare disruption types and measured waiting/closure severity. Those specifications are supplementary and are not alternative estimates of exactly the same exposure contrast.

5 Results

5.1 Persistent participation responses

[Figure 2](#) reports the event-study coefficients for the three turnout outcomes. The most stable result is a negative in-person coefficient at every subsequent election. Relative to the 2019 European reference, the point estimates are -1.15 percentage points in the 2023 state rerun, -2.33 in the 2024 European election, -1.99 in the 2025 federal election, and -1.27 in the 2026 state election. All four are statistically significant under baseline precinct-clustered inference. The 2021 state and federal observations refer to the same election day and should not be counted as independent instances of failure. The negative difference persisting through subsequent regular elections is the main empirical evidence that the participation response outlasted the original administrative event.

The size of the in-person difference is economically meaningful relative to the underlying mode-specific participation rate. The 2024 and 2025 point estimates correspond to roughly two percentage points, or about five percent of average in-person turnout in the harmonized panel. This does not imply an equivalent reduction in the probability that a voter participates at all, because citizens may use an alternative channel. Postal turnout is therefore essential

to interpreting the result.

Postal coefficients are negative but imprecisely estimated in the concurrent 2021 returns, approximately zero in the 2023 state rerun, and positive in 2024 and 2025: 1.20 and 1.19 percentage points, respectively, both statistically significant. The positive estimate in 2026 (0.43 percentage points, SE 0.31) is less precise. The 2024–2025 coefficients are consistent with a gradual adjustment toward postal voting in the affected local electorates, although the postal responses are not monotonic across elections. The 2021 postal coefficients should not be interpreted as behavioral reactions to later election-day queues, because many postal votes were cast beforehand.

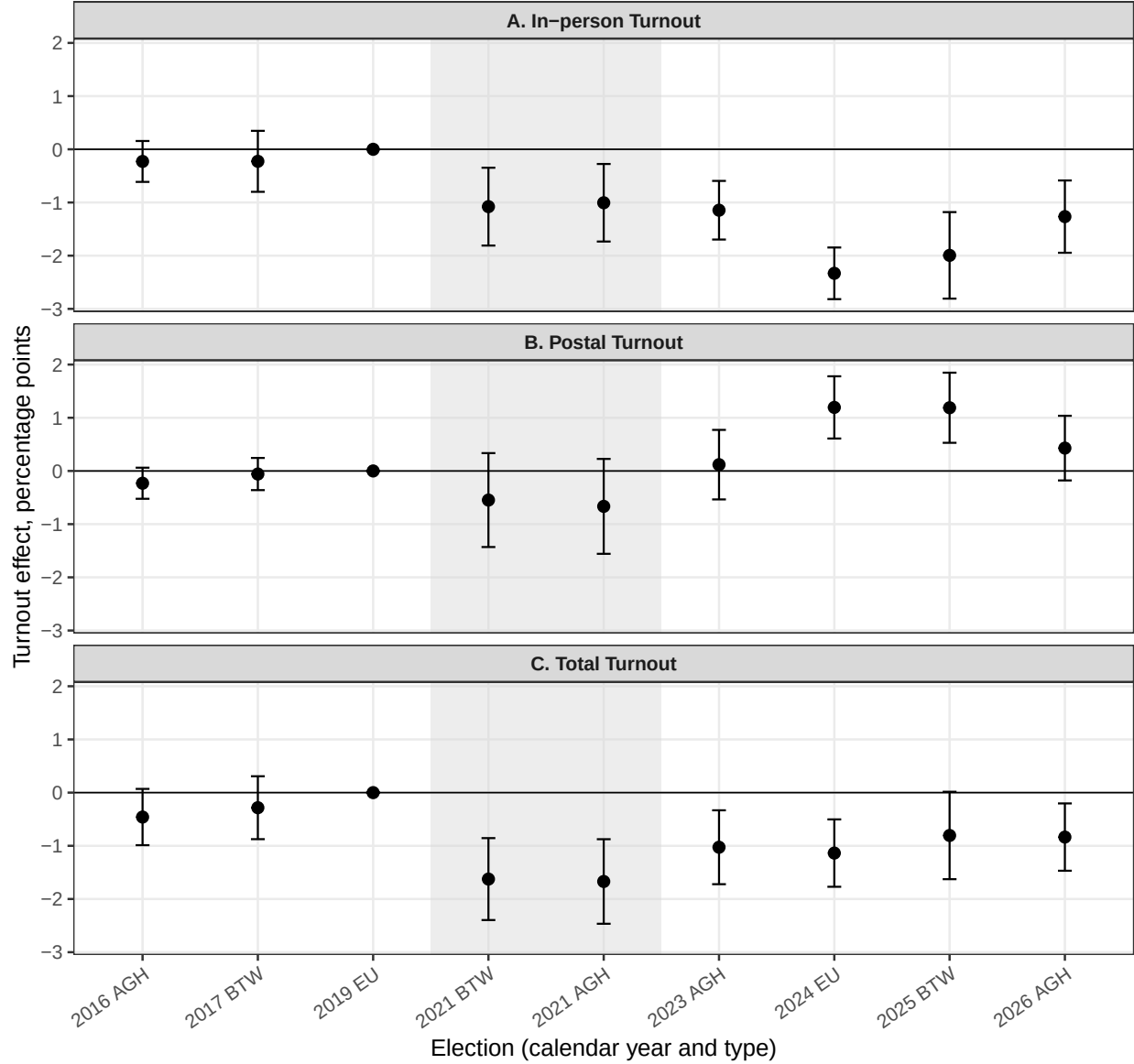
The estimated postal increases in 2024 and 2025 amount to approximately 51 and 60 percent, respectively, of the contemporaneous in-person declines. This comparison is an accounting ratio of model coefficients, not the fraction of individuals who switched mode; it is reported descriptively, without attaching a precision estimate to the ratio itself. Both channels enter the same turnout denominator, so their coefficients sum to the total-turnout estimate. The corresponding total coefficients are -1.03 in 2023, -1.14 in 2024, -0.81 in 2025, and -0.84 percentage points in 2026. In the pooled model the total-turnout differences remain negative, but their precision and stability are weaker than for in-person turnout. This distinction is central: the evidence more consistently indicates a persistent redistribution of participation away from polling places than a precisely measured permanent increase in abstention.

5.2 Voting-mode adjustment within election types

The pooled estimates compare changes across federal, state, and European elections, each with different turnout levels and electoral stakes. Separate within-election-family estimates provide an informative comparison without treating these elections as otherwise identical. [Table 1](#) reports the 2023 and 2026 state-election estimates relative to 2016 AGH and the 2025 federal estimate relative to 2017 federal. The full set of within-family estimates, including 2021 and 2024, is provided in [Appendix Table C1](#).

The 2025 federal comparison is particularly informative about the distinction between mode substitution and abstention. The in-person coefficient is -1.77 percentage points (SE 0.27), while postal turnout is 1.25 points higher (SE 0.37). The net total-turnout coefficient is -0.52 points (SE 0.37) and is not statistically distinguishable from zero. This is compatible with substantial compensation through postal participation, not proof that the initial participation loss was fully reversed. Both federal comparison elections were conducted under an 18+ franchise, which removes one concern present in the 2026 state comparison.

Figure 2: Turnout and voting-mode adjustment after the 2021 administrative failure



Notes: The figure presents event-study estimates based on [Equation 4](#). Outcomes are in-person, postal, and total turnout, measured in percentage points of eligible voters. The reference election is the 2019 European election. The two 2021 outcomes are concurrent federal and state ballots from the same election day and should not be interpreted as independent exposure events. The 2019 European election is normalized to zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by 2021 postal precinct. The companion regression table is reported in [Appendix Table B1](#).

Table 1: Voting-mode adjustment within comparable election types

Election	Reference	In-person turnout	Postal turnout	Total turnout
2023 AGH	2016 AGH	-0.917*** (0.259)	0.349 (0.345)	-0.568 (0.353)
2025 Bundestag	2017 Bundestag	-1.768*** (0.269)	1.246*** (0.367)	-0.522 (0.371)
2026 AGH	2016 AGH	-1.037*** (0.286)	0.661 ⁺ (0.348)	-0.377 (0.311)

Notes: Coefficients are percentage-point differences relative to the omitted election within each family: 2016 AGH for state elections and 2017 federal for federal elections. Standard errors (in parentheses) are clustered by 2021 postal precinct. Models include precinct and election-by-district fixed effects. The 2026 AGH electorate includes German citizens aged 16–17, unlike the 2016 reference. The complete results, including 2021 and European elections, appear in [Appendix Table C1](#).

The 2023 state rerun shows an in-person deficit of -0.92 points (SE 0.26) relative to 2016 AGH, with less precise postal and total estimates. In 2026, the in-person estimate remains negative (-1.04 , SE 0.29), while the total-turnout estimate is -0.38 (SE 0.31). A statistically insignificant total effect permits a range of aggregate responses and is not evidence of zero abstention costs. The 2026 comparison also crosses the expansion of the state electorate to 16- and 17-year-olds. Consequently, the longer horizon supports persistence in the in-person participation difference more convincingly than it establishes a lasting net total-turnout loss.

5.3 Identification and robustness

Baseline differences and counterfactual trends. Court-linked exposure is not randomized. In 2019, the treated precincts averaged about 1,884 residents, compared with 1,642 in the clean controls, and differed in age and socioeconomic composition ([Appendix Table B2](#)). Precinct fixed effects remove stable level differences; election-by-district effects account for common district-level election shocks. Neither removes treatment-specific trends arising from changes in local populations or administration. The two pre-treatment elections offer a limited diagnostic: their in-person coefficients are small relative to later differences, but their election types differ and the 2016 total-turnout estimate is not negligible relative to some later aggregate effects ([Appendix Table B3](#)). These facts constrain a causal interpretation even when the post-2021 estimates are precise.

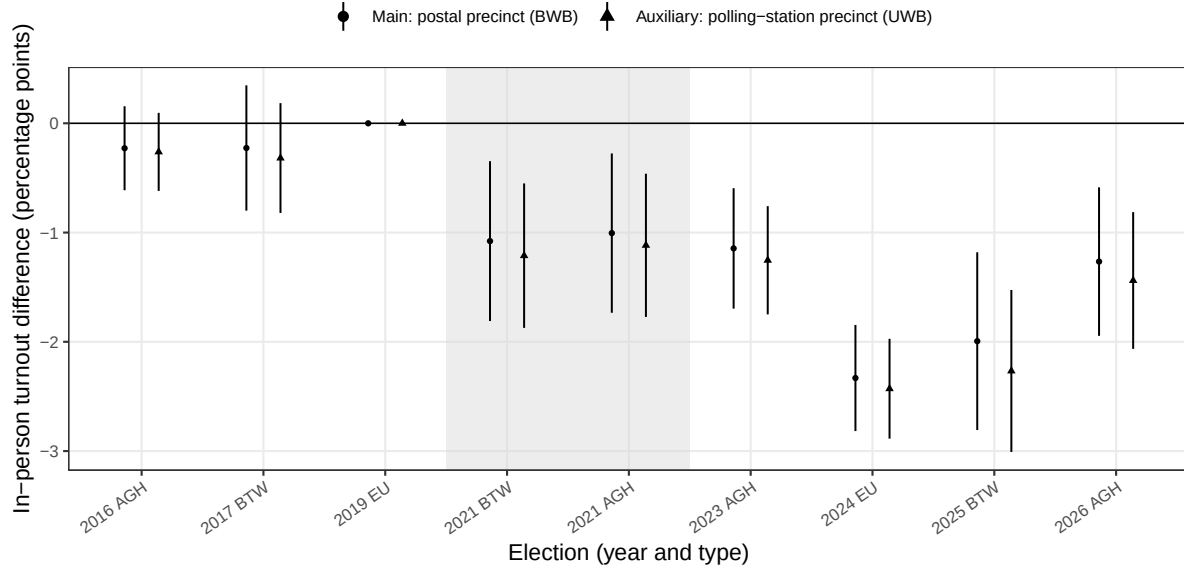
The preferred in-person estimates remain negative after adding socioeconomic and baseline controls, excluding district 07, removing adjacent controls, and using matching procedures ([Appendix Table C2](#); [Appendix Table C3](#); [Appendix Table C4](#); [Appendix Table C5](#)). The placebo-reassignment and district-clustered inference checks discussed in [Section 4.2](#)

strengthen the assessment of statistical uncertainty, without making court-linked exposure random.

Geographic robustness. The harmonized postal-precinct panel combines votes originating in different reporting geographies. Models with additional socioeconomic and baseline controls leave the post-2021 in-person pattern negative ([Appendix Figure C1](#); [Appendix Table C2](#)). Crosswalk-concentration weighting and restriction to the k90 sample produce comparable in-person trajectories ([Appendix Figure C5](#); [Appendix Table C6](#)). These estimates change the weights or sample and are not identical estimands.

The auxiliary analysis on the 2021 polling-station-precinct geography provides a separate check on the in-person outcome. Figure 3 compares its full event-study path with the main 2021 postal-precinct estimates; the estimated declines are similar across both geographies and persist through 2026 ([Appendix Table C7](#)). This is stronger geographic validation for *in-person* participation than for postal or total turnout, which still require allocations to common BWB units. The harmonized panel includes 18 precinct-election observations with total turnout above 100 percent, most in the 2021 returns. A re-estimation excluding these observations leaves the 2026 in-person coefficient effectively unchanged (-1.266 percentage points, SE 0.346 in both samples; [Appendix Table C8](#)). This check is reported for the terminal 2026 contrast; it does not establish that all earlier event-study coefficients or spatial allocations are unaffected. The 2024 European and 2026 state electorates also include newly eligible 16–17-year-olds, whereas the common raster measures German citizens aged 18 and older. The latter mismatch affects spatial allocation, not the official election-specific electorate denominator.

Figure 3: In-person turnout responses across geographic constructions



Notes: The figure compares the main event-study estimates on 2021 postal-precinct geography (BWB) with auxiliary estimates on 2021 polling-station-precinct geography (UWB). Both outcomes are in-person turnout in percentage points of eligible voters, with the 2019 European election as the reference. Symbols show point estimates and bars show 95% confidence intervals clustered at the respective 2021 unit. The shaded band marks the two concurrent 2021 ballots; they are not separate disruption events. The geographic constructions have different units and are not identical estimands. Full auxiliary coefficients appear in [Appendix Table C7](#).

5.4 Institutional aftermath and supporting evidence

The subsequent turnout responses reflect the 2021 breakdown and its legal aftermath, including the full 2023 state rerun and the geographically targeted 2024 federal rerun. The design cannot isolate the election-day disruption from these later institutional events.

Disruption-family estimates offer limited evidence on mechanisms. Direct-access and ballot-error-only precincts differ significantly in their 2024 in-person response (-1.76 percentage points; $p = 0.004$), but not individually in other follow-up elections. Negative contemporaneous postal coefficients in the access-cost group, attenuated when excluding district 07, further complicate interpretation ([Appendix Table D2](#); [Appendix Table D3](#)). The severity results likewise remain exploratory ([Appendix Table D5](#)). These estimates do not distinguish learning, habit change, or revised expectations of voting costs.

Among 622 originally shared polling sites, there is no precise relative decongestion effect in 2026 ($+6.1$ percentage points; SE 5.2), while the probability of a different nearby site is higher ($+8.9$ points; SE 4.1). These are administrative changes, not identified mediators of turnout ([Appendix Section E](#); [Appendix Figure E1](#); [Appendix Figure E2](#); [Appendix Table E1](#)).

Survey responses provide contextual evidence on public salience ([Appendix Section F](#)).

6 Conclusion

Berlin’s 2021 electoral breakdown and its subsequent legal aftermath provide evidence on persistent changes in local voting-mode composition. Court-linked affected local electorates display persistently lower in-person turnout across subsequent elections, including 2026. Postal participation increases significantly in the 2024 European and 2025 federal elections, providing evidence of partial compensation through an available alternative channel. The estimated total-turnout penalty is smaller and more sensitive to the election family and geographic specification. Persistent reductions in polling-place participation should therefore not be equated mechanically with persistent abstention.

The distinction extends work on contemporaneous voting costs and downstream participation by separating the voting channel from the decision to participate. Administrative failures can matter even after formal electoral procedures resume, and the participation response can operate through changes in the mode by which citizens vote. The 2025 within-federal comparison illustrates this possibility without the 2026 state-election franchise expansion. The result is consistent with changing expected relative costs and with costly adjustment to postal voting, but the precinct panel cannot identify individual switching, belief updating, or a unique habit-formation mechanism. Nonrandom court-linked exposure and imperfect spatial harmonization also remain relevant to causal interpretation.

The findings are consistent with alternative voting channels helping to accommodate changed participation behavior without restoring the earlier mode composition. The institutional implication concerns both the immediate reliability of voting administration and the conditions under which citizens participate in later elections. The evidence does not identify the effect of providing postal voting or of the legal reruns. It establishes a persistent place-level association with the institutional episode under a conditional parallel-trends assumption, not a directly measured persistence in subjective voting costs.

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A Electoral institutions, treatment records, and spatial measurement

German federal and Berlin state elections use personalized proportional representation. Citizens receive election notifications identifying their assigned polling location and may request postal ballots for each election. Voting by post is an established option, but requesting, completing, and returning the documents must take place within election-specific deadlines. These administrative distinctions motivate separate treatment of election-day and postal ballot counts in the empirical panel.

The treatment taxonomy draws on two partially overlapping court sources. Federal-linked exposure defines the principal treatment contrast; state-linked records additionally classify documented queues, interruptions, missing ballots, and ballot errors. A unit appearing only in the state record is not included as a clean federal control. The descriptive counts below establish how much the primary treatment and the state-record mechanism groups overlap. They do not establish that every voter residing in a classified precinct experienced the reported event.

The panel combines results originally reported on separate polling-station and postal-precinct geographies. For each source precinct s and 2021 target precinct i , a population-weighted crosswalk assigns a nonnegative share w_{si} , with $\sum_i w_{si} = 1$ for each source. A source-level count V_{st} is allocated as $\tilde{V}_{it} = \sum_s w_{si} V_{st}$, using distinct crosswalks for in-person and postal sources. Turnout rates use correspondingly allocated official election-specific registered-voter counts. By construction, source counts are preserved when the weights sum to one. Conservation does not independently validate the distribution of allocated counts within Berlin.

The underlying 2022 grid measures German citizens aged 18 and older. This is a constant proxy for the geographical distribution of eligible voters, not a claim that all election-specific franchises are identical. The 2024 European and 2026 state franchises include eligible 16–17-year-olds, while the former also includes non-German EU citizens. Interpolation precision may vary with the extent of boundary overlap and spatial population composition. Appendix crosswalk tests therefore report both mapping complexity and regression sensitivity. A true 16+-matched sensitivity would require verified fine-resolution population counts; none are imputed. In figure axes and regression tables, AGH denotes the Berlin state election, BTW the federal Bundestag election, and EU the European Parliament election. The displays first document source exposure, summary statistics, and spatial allocation, followed by the detailed state-record disruption taxonomy.

Table A1: Source-overlap counts

Source category	Postal precincts	Share
No court record	1056	0.701
Federal record only	17	0.011
State record only	157	0.104
Federal and state records	277	0.184

Notes: The table reports counts of 2021 postal precincts by federal and state court-record source category. Clean controls are precincts not mentioned in either record. The corresponding figure is [Figure A1](#).

Table A2: Summary statistics

Variable	N	Mean	SD	Min	25th pct.	Median	75th pct.	Max
AfD votes (eligible-voter share)	13563	7.748	4.761	0.402	4.216	6.772	10.188	30.872
AfD vote share (valid votes)	13563	11.171	7.071	0.587	5.765	9.560	15.185	48.865
CDU votes (eligible-voter share)	13563	13.826	7.524	1.415	7.802	12.567	18.563	45.714
CDU vote share (valid votes)	13563	19.191	9.504	2.055	11.458	18.240	25.735	58.458
Eligible voters	13563	1641.982	607.029	247.190	1149.902	1391.029	2120.187	3998.704
FDP votes (eligible-voter share)	13563	4.263	3.072	0.000	2.026	3.387	5.637	25.820
FDP vote share (valid votes)	13563	5.772	3.676	0.000	3.039	4.808	7.649	31.014
Green votes (eligible-voter share)	13563	13.384	8.201	0.473	6.651	11.981	18.762	47.233
Green vote share (valid votes)	13563	18.385	10.511	1.411	9.683	16.697	25.472	53.761
Left votes (eligible-voter share)	13563	10.748	7.393	0.438	4.769	9.436	14.782	49.991
Left vote share (valid votes)	13563	14.938	9.728	0.724	6.900	13.346	20.915	62.693
In-person turnout (eligible-voter share)	13563	42.695	6.919	19.181	37.988	42.615	47.755	68.818
Postal turnout (eligible-voter share)	13563	28.742	8.976	5.036	22.019	28.373	34.956	86.934
SPD votes (eligible-voter share)	13563	12.439	4.242	2.641	9.268	12.095	15.430	31.265
SPD vote share (valid votes)	13563	17.387	5.250	4.267	13.663	16.901	20.946	41.628
Total turnout (eligible-voter share)	13563	71.437	11.135	30.226	64.528	72.518	79.651	126.071
Residents	13563	1696.469	350.535	557.000	1459.000	1699.000	1907.000	3109.699
Age 18-25 share	13563	7.826	2.446	2.417	6.210	7.531	8.992	25.338
Age 25-35 share	13563	16.158	7.047	3.554	10.879	15.183	20.095	51.989
Age 35-45 share	13563	16.527	5.741	5.767	12.451	14.956	19.297	36.423
Age 45-60 share	13563	24.903	4.681	10.492	21.756	24.772	27.787	42.202
Age 60-70 share	13563	14.264	4.020	2.292	11.845	14.568	16.729	31.019
Age 65+ share	13563	20.451	8.929	1.176	13.470	20.894	26.533	60.700
Age 70+ share	13563	20.321	9.608	1.146	12.772	20.796	26.960	59.774
Migration background (share)	13563	13.844	8.104	1.864	7.833	12.155	18.208	63.429
SGB II recipients (share)	13563	13.272	9.413	0.000	5.927	10.850	18.729	56.299
Foreign citizens (share)	13563	19.224	10.131	0.867	11.053	18.240	26.495	53.317

Notes: The table reports summary statistics for the analysis panel. Variables include turnout outcomes, selected party vote-share outcomes, and available socio-demographic controls. Variable labels are reader-facing labels rather than raw R column names.

Table A3: Crosswalk quality summary

Election	Geometry	Source units	Mean weight sum	Min weight sum	Max targets/source	Mean targets/source
2016 State	Polling-station precinct to BWB21	1779	1	1	18	4.997
2016 State	Postal precinct to BWB21	653	1	1	28	8.763
2017 Federal	Polling-station precinct to BWB21	1779	1	1	16	4.187
2017 Federal	Postal precinct to BWB21	660	1	1	22	7.771
2019 European	Polling-station precinct to BWB21	1800	1	1	16	4.173
2019 European	Postal precinct to BWB21	718	1	1	19	7.400
2024 European	Polling-station precinct to BWB21	2220	1	1	7	1.613
2024 European	Postal precinct to BWB21	1205	1	1	8	2.390
2025 Federal	Polling-station precinct to BWB21	2323	1	1	6	1.467
2025 Federal	Postal precinct to BWB21	1275	1	1	6	2.107
2026 State	Polling-station precinct to BWB21	2542	1	1	15	4.366
2026 State	Postal precinct to BWB21	1572	1	1	17	5.574

Notes: The table summarizes population-weighted crosswalks from historical election geographies to 2021 postal precincts. It documents the harmonization process used to place in-person, postal, and total turnout on a common geography.

Table A4: Audit of harmonized turnout above 100 percent

outcome	Observations	Observations \$>\$ 100	Share \$>\$ 100	Maximum
In-person turnout (eligible-voter share)	13563	0	0.000	68.818
Postal turnout (eligible-voter share)	13563	0	0.000	86.934
Total turnout (eligible-voter share)	13563	18	0.001	126.071

Notes: The table counts harmonized precinct-election observations with turnout above 100 percent. These rare observations can reflect allocation of election-specific votes and registered-voter denominators through different source geometries; additional source-level checks are required and motivate the harmonization checks in [Figure C5](#) and [table C6](#).

Table A5: State-record disruption-family counts

State-record disruption family	Postal precincts	Share
Direct access-cost only	357	0.823
Access-cost plus ballot error	35	0.081
Ballot problem only	42	0.097

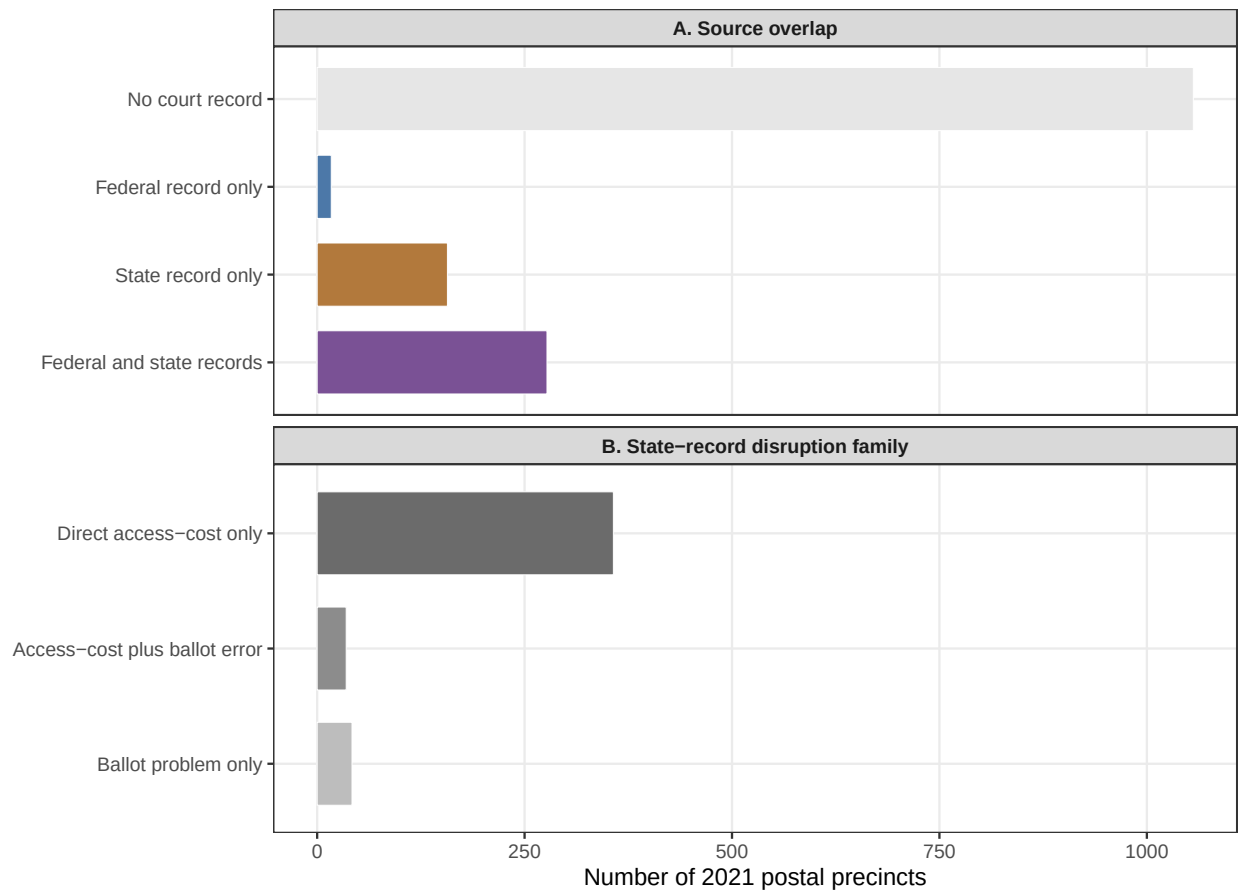
Notes: The table reports counts of state-record precincts by broad disruption family. Missing ballots are coded as access/cost disruptions because they prevent or delay normal in-person voting.

Table A6: Full state-record disruption taxonomy counts

Source category	Detailed state-record category	Broad family	Postal precincts	Share within source
Federal and state records	Missing ballots	Direct access-cost only	1	0.004
Federal and state records	Waiting/closure plus missing ballots	Direct access-cost only	12	0.043
Federal and state records	Waiting/closure/disruption	Direct access-cost only	232	0.838
Federal and state records	Access-cost plus ballot error	Access-cost plus ballot error	32	0.116
State record only	Missing ballots	Direct access-cost only	57	0.363
State record only	Waiting/closure plus missing ballots	Direct access-cost only	4	0.025
State record only	Waiting/closure/disruption	Direct access-cost only	51	0.325
State record only	Access-cost plus ballot error	Access-cost plus ballot error	3	0.019
State record only	Ballot error only	Ballot problem only	42	0.268

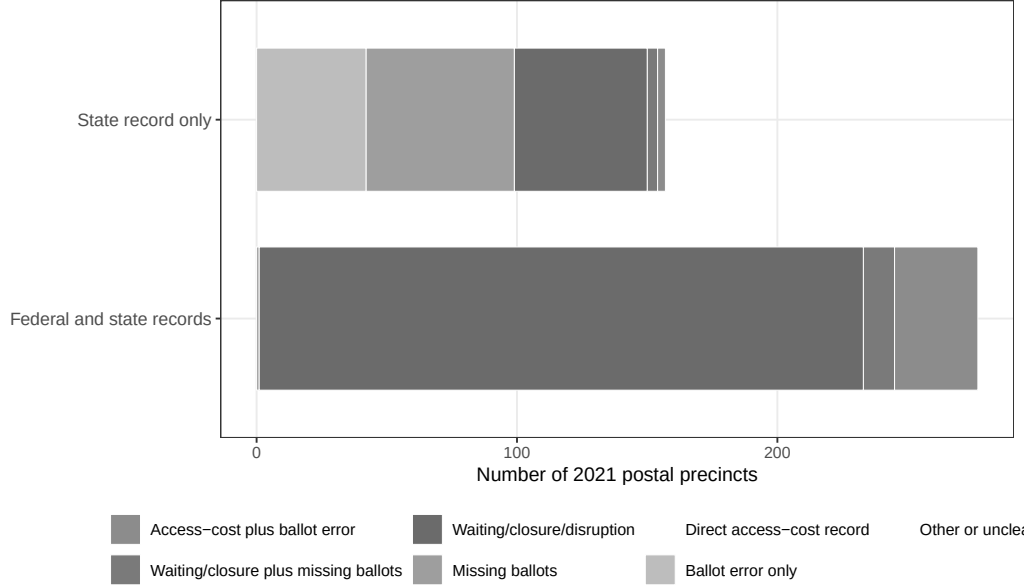
Notes: The table reports the detailed state-record disruption taxonomy underlying [Figure A2](#).

Figure A1: Source overlap and state-record disruption families



Notes: Panel A counts 2021 postal precincts by overlap between federal and state court-linked records. Panel B counts state-record precincts by broad disruption family. Direct access/cost disruptions include waiting, voting after closing, closures or interruptions, and missing ballots. Ballot-problem-only records capture ballot errors without direct access/cost disruption. The companion tables are [Tables A1](#) and [A5](#).

Figure A2: Full state-record disruption taxonomy



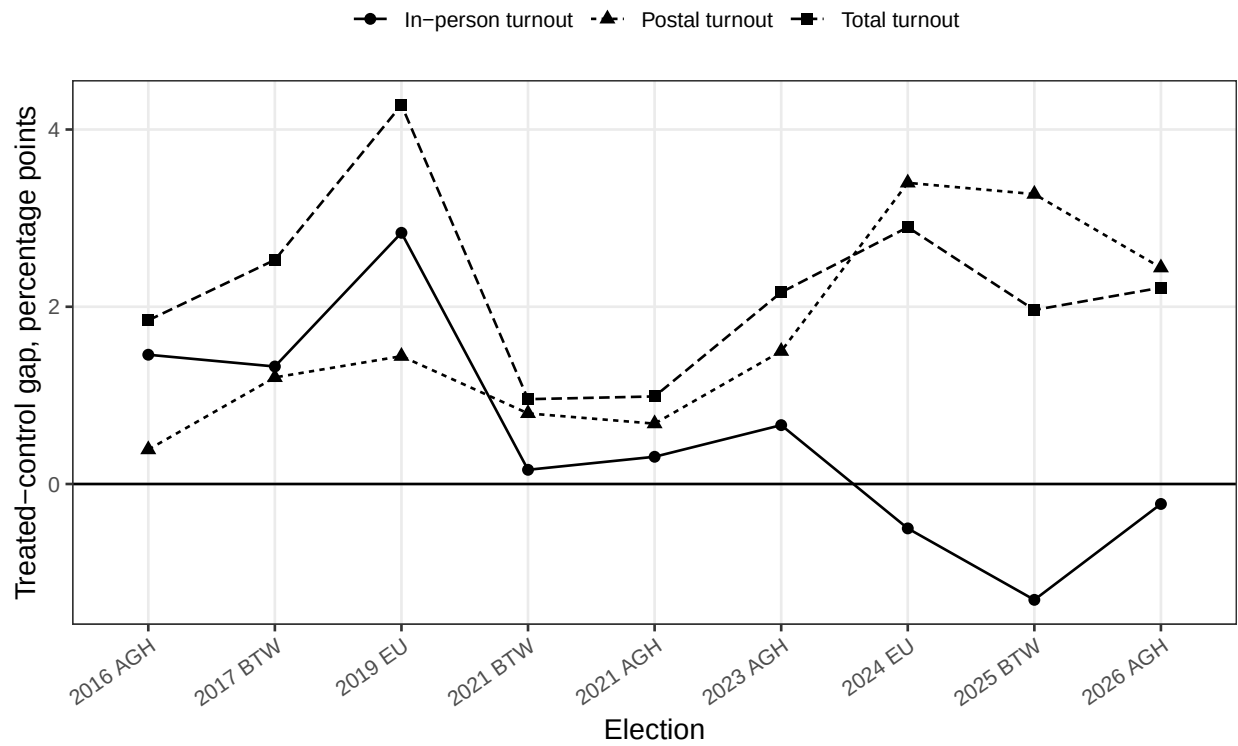
Notes: The figure reports the detailed state-record disruption taxonomy. It is descriptive and clarifies the construction of the broad disruption-family categories used in [Figure D1](#). The corresponding counts are reported in [Table A6](#).

B Identification and baseline diagnostics

The preferred design compares changes in court-linked treated and clean-control postal precincts relative to the 2019 European election, conditional on precinct and election-by-district fixed effects. Exposure is documented rather than randomized. The descriptive turnout gaps, baseline estimates, and 2019 balance statistics document the initial empirical comparison. Pre-treatment coefficients, placebo reassignment, and alternative inference assess its credibility without treating exposure as random.

Only two non-reference pre-treatment elections are observed in the harmonized panel, and they belong to different election families. The diagnostics consequently provide limited leverage over possible differential trends. Randomized placebo assignments preserve treatment counts within districts for an inferential benchmark, not a claim that the court-linked designation was randomized. The table comparing post-treatment coefficients with the largest observed pre-treatment coefficient is a descriptive calibration and not a formal bound on confounding or an HonestDiD identified set.

Figure B1: Treated-control turnout gaps by election and voting mode



Notes: The figure plots treated-control differences in in-person, postal, and total turnout across elections. Turnout is measured as a percentage of eligible voters. The treatment is the main federal court-linked exposure indicator. The figure is descriptive and does not condition on postal-precinct fixed effects or election-by-district fixed effects.

Table B1: Main specification: turnout levels

Election	(1) In-person turnout	(2) Postal turnout	(3) Total turnout
2016 AGH	-0.228 (0.196)	-0.231 (0.149)	-0.459 ⁺ (0.270)
2017 federal	-0.226 (0.292)	-0.058 (0.154)	-0.284 (0.301)
2021 federal	-1.078** (0.373)	-0.547 (0.450)	-1.625*** (0.393)
2021 AGH	-1.005** (0.372)	-0.666 (0.455)	-1.671*** (0.405)
2023 AGH	-1.145*** (0.281)	0.118 (0.333)	-1.027** (0.354)
2024 European	-2.332*** (0.247)	1.195*** (0.298)	-1.137*** (0.323)
2025 federal	-1.994*** (0.415)	1.188*** (0.336)	-0.806 ⁺ (0.419)
2026 AGH	-1.266*** (0.346)	0.430 (0.310)	-0.836** (0.323)
<i>Fixed-effects</i>			
Precinct	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	12,150	12,150	12,150

Notes: The table presents event-study results based on [Equation 4](#). Columns (1), (2), and (3) report in-person, postal, and total turnout, respectively. Outcomes are measured in percentage points of eligible voters. The reference election is the 2019 European election. All specifications include 2021 postal-precinct fixed effects and election-by-district fixed effects. The corresponding figure is [Figure 2](#).

Table B2: Pre-treatment characteristics by court-linked exposure, 2019 EU

Characteristic	Treated mean	Control mean	Std. difference	Treated N	Control N
Residents	1883.577	1642.360	0.743	294	1056
Foreign-citizen share	20.283	18.528	0.178	294	1056
Migration-background share	13.622	13.796	-0.022	294	1056
SGB II share	11.413	13.628	-0.242	294	1056
Age 18-25 share	7.806	7.842	-0.014	294	1056
Age 25-35 share	16.744	15.613	0.170	294	1056
Age 35-45 share	17.817	15.817	0.354	294	1056
Age 45-60 share	26.475	24.579	0.403	294	1056
Age 60-70 share	13.127	14.758	-0.422	294	1056
Age 65+ share	18.075	21.591	-0.399	294	1056
Age 70+ share	18.032	21.390	-0.351	294	1056

Notes: The table reports 2019 European-election pre-treatment characteristics for the preferred treated and clean-control sample. Standardized differences equal the treated-minus-control mean divided by the square root of the average group variance. The comparisons are descriptive; district-adjusted treatment-prediction regressions are retained separately in the replication outputs.

Table B3: Pretrend and placebo diagnostics

Outcome	Election	Estimate	Std. error	95% CI lower	95% CI upper	p-value
In-person turnout	2016 AGH	-0.228	0.196	-0.612	0.156	0.244
In-person turnout	2017 federal	-0.226	0.292	-0.799	0.347	0.439
Postal turnout	2016 AGH	-0.231	0.149	-0.523	0.061	0.121
Postal turnout	2017 federal	-0.058	0.154	-0.360	0.245	0.709
Total turnout	2016 AGH	-0.459	0.270	-0.988	0.069	0.089
Total turnout	2017 federal	-0.284	0.301	-0.874	0.307	0.347

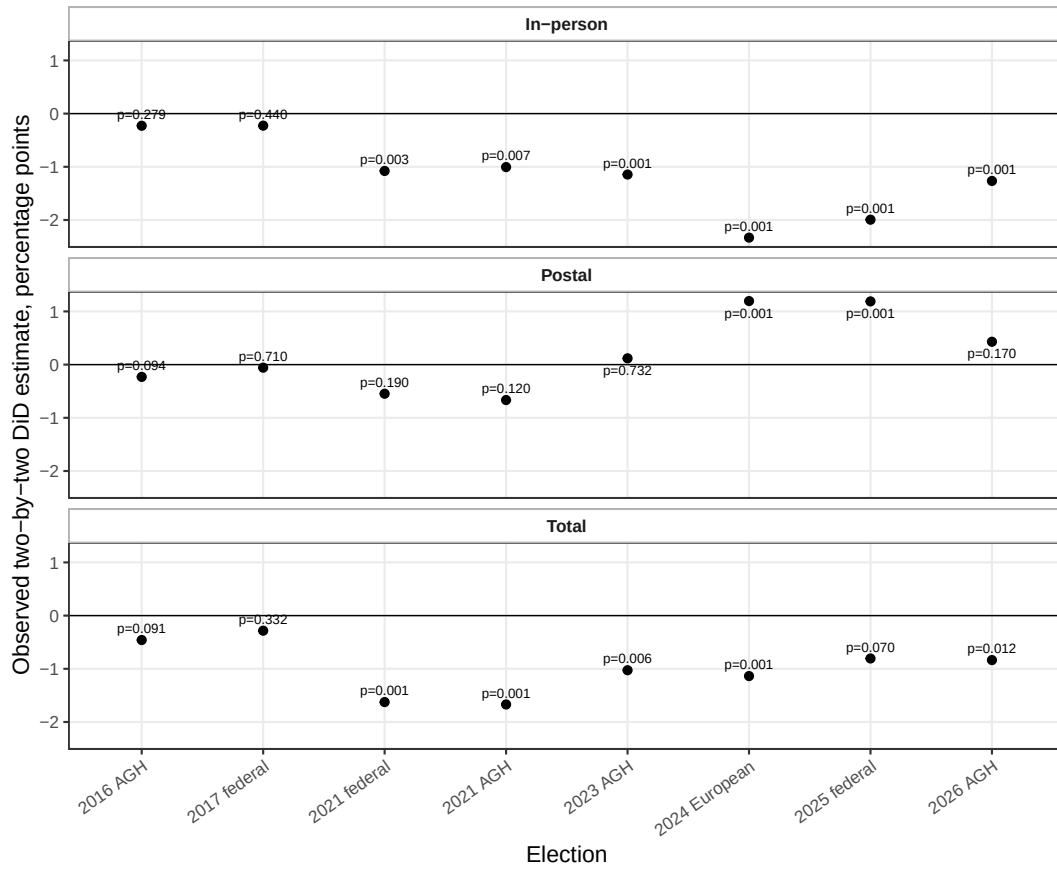
Notes: The table reports pre-treatment and placebo diagnostics for the main event-study design. The reference election is the 2019 European election.

Table B4: Dynamic randomization inference by election and turnout mode

Election vs. 2019 reference	In-person turnout	Postal turnout	Total turnout
2016 AGH	-0.228 [p=0.279]	-0.231 [p=0.094]	-0.459 [p=0.091]
2017 federal	-0.226 [p=0.440]	-0.058 [p=0.710]	-0.284 [p=0.332]
2021 federal	-1.078 [p=0.003]	-0.547 [p=0.190]	-1.625 [p=0.001]
2021 AGH	-1.005 [p=0.007]	-0.666 [p=0.120]	-1.671 [p=0.001]
2023 AGH	-1.145 [p=0.001]	0.118 [p=0.732]	-1.027 [p=0.006]
2024 European	-2.332 [p=0.001]	1.195 [p=0.001]	-1.137 [p=0.001]
2025 federal	-1.994 [p=0.001]	1.188 [p=0.001]	-0.806 [p=0.070]
2026 AGH	-1.266 [p=0.001]	0.430 [p=0.170]	-0.836 [p=0.012]

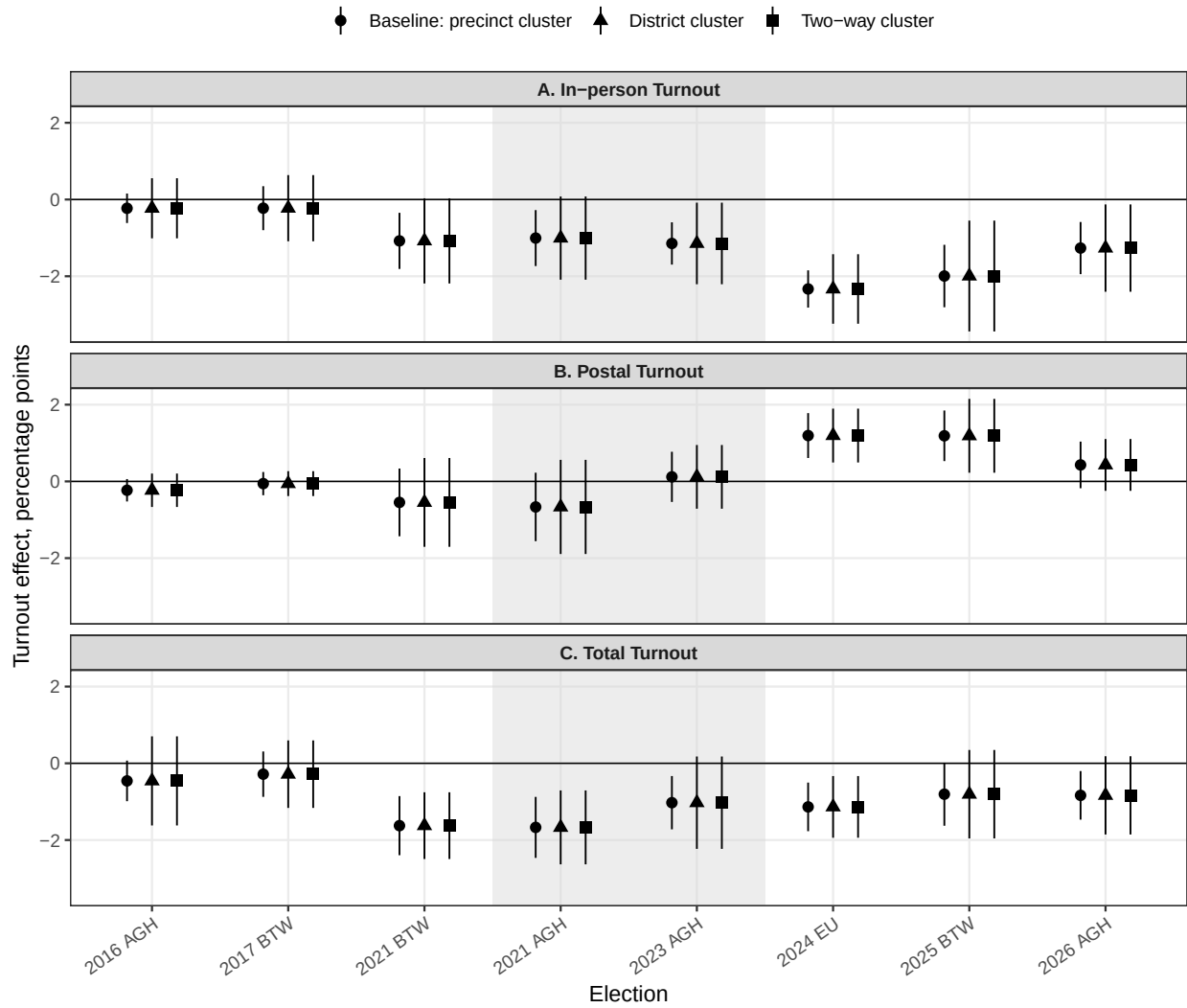
Notes: Each row compares the listed election to the 2019 European reference election in a two-period difference-in-differences specification. Treatment is randomly reassigned across 2021 postal precincts while preserving the number of treated precincts within districts. The corresponding figure is [Figure B2](#).

Figure B2: Dynamic randomization inference



Notes: The figure compares the observed event-specific treatment estimates to placebo assignments that preserve the number of treated 2021 postal precincts within districts. The exercise is run separately by election and turnout mode, each relative to the 2019 European reference election. The companion table is [Table B4](#).

Figure B3: Alternative standard errors and clustering



Notes: The figure compares the baseline precinct-clustered specification to alternative inference choices. The reference election is the 2019 European election. The companion table is [Table B6](#).

Table B5: Descriptive calibration using pre-treatment coefficients

Outcome	Election	Estimate	Std. error	Max. absolute pre-period coefficient	Post/pre magnitude ratio
In-person turnout	2021 federal	-1.078	0.373	0.228	4.722
In-person turnout	2021 AGH	-1.005	0.372	0.228	4.402
In-person turnout	2023 AGH	-1.145	0.281	0.228	5.016
In-person turnout	2024 European	-2.332	0.247	0.228	10.211
In-person turnout	2025 federal	-1.994	0.415	0.228	8.733
In-person turnout	2026 AGH	-1.266	0.346	0.228	5.543
Postal turnout	2021 federal	-0.547	0.450	0.231	2.370
Postal turnout	2021 AGH	-0.666	0.455	0.231	2.882
Postal turnout	2023 AGH	0.118	0.333	0.231	0.513
Postal turnout	2024 European	1.195	0.298	0.231	5.174
Postal turnout	2025 federal	1.188	0.336	0.231	5.147
Postal turnout	2026 AGH	0.430	0.310	0.231	1.861
Total turnout	2021 federal	-1.625	0.393	0.459	3.539
Total turnout	2021 AGH	-1.671	0.405	0.459	3.638
Total turnout	2023 AGH	-1.027	0.354	0.459	2.236
Total turnout	2024 European	-1.137	0.323	0.459	2.475
Total turnout	2025 federal	-0.806	0.419	0.459	1.754
Total turnout	2026 AGH	-0.836	0.323	0.459	1.820

Notes: Post-treatment coefficient magnitudes are compared with the largest absolute 2016/2017 coefficient for each outcome. Ratios are descriptive, election-specific calibrations; they are not an HonestDiD identified set or a proof of parallel trends. The ratio compares each post-treatment coefficient to the largest observed pre-treatment coefficient in the same outcome; it does not model or bound post-treatment confounding.

Table B6: Alternative standard errors and clustering

<i>Panel A: In-person turnout</i>			
Election	(1) precinct cluster	(2) district cluster	(3) two-way cluster
2016 AGH	-0.228 (0.196)	-0.228 (0.356)	-0.228 (0.356)
2017 federal	-0.226 (0.292)	-0.226 (0.392)	-0.226 (0.392)
2021 federal	-1.078** (0.373)	-1.078+ (0.504)	-1.078+ (0.504)
2021 AGH	-1.005** (0.372)	-1.005+ (0.492)	-1.005+ (0.492)
2023 AGH	-1.145*** (0.281)	-1.145* (0.483)	-1.145* (0.483)
2024 European	-2.332*** (0.247)	-2.332*** (0.411)	-2.332*** (0.411)
2025 federal	-1.994*** (0.415)	-1.994* (0.656)	-1.994* (0.656)
2026 AGH	-1.266*** (0.346)	-1.266* (0.517)	-1.266* (0.517)
<i>Panel B: Postal turnout</i>			
Election	(1) precinct cluster	(2) district cluster	(3) two-way cluster
2016 AGH	-0.231 (0.149)	-0.231 (0.198)	-0.231 (0.198)
2017 federal	-0.058 (0.154)	-0.058 (0.147)	-0.058 (0.147)
2021 federal	-0.547 (0.450)	-0.547 (0.526)	-0.547 (0.526)
2021 AGH	-0.666 (0.455)	-0.666 (0.557)	-0.666 (0.557)
2023 AGH	0.118 (0.333)	0.118 (0.377)	0.118 (0.377)
2024 European	1.195*** (0.298)	1.195** (0.319)	1.195** (0.319)
2025 federal	1.188*** (0.336)	1.188* (0.437)	1.188* (0.437)
2026 AGH	0.430 (0.310)	0.430 (0.307)	0.430 (0.307)
<i>Panel C: Total turnout</i>			
Election	(1) precinct cluster	(2) district cluster	(3) two-way cluster
2016 AGH	-0.459+ (0.270)	-0.459 (0.528)	-0.459 (0.528)
2017 federal	-0.284 (0.301)	-0.284 (0.400)	-0.284 (0.400)
2021 federal	-1.625*** (0.393)	-1.625** (0.395)	-1.625** (0.395)
2021 AGH	-1.671*** (0.405)	-1.671** (0.438)	-1.671** (0.438)
2023 AGH	-1.027** (0.354)	-1.027+ (0.547)	-1.027+ (0.547)
2024 European	-1.137*** (0.323)	-1.137** (0.365)	-1.137** (0.365)
2025 federal	-0.806+ (0.419)	-0.806 (0.524)	-0.806 (0.524)
2026 AGH	-0.836** (0.323)	-0.836+ (0.463)	-0.836+ (0.463)
<i>Fixed-effects</i>			
Precinct	Yes	Yes	Yes
Election × District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	12,150	12,150	12,150

Notes: The table re-estimates the main event-study specification under alternative inference choices. The reference election is the 2019 European election. The corresponding figure is [Figure B3](#).

C Robustness of the participation response

The robustness checks target several separate threats. Within-election-type models examine whether the pooled estimates are sensitive to combining AGH, Bundestag, and European elections. Additional controls and matching address observable baseline differences; exclusion of district 07 and adjacent clean controls addresses particular reporting concerns and potential local contamination. Alternative geographic weights and the original polling-station-precinct analysis assess whether crosswalk construction drives the in-person pattern.

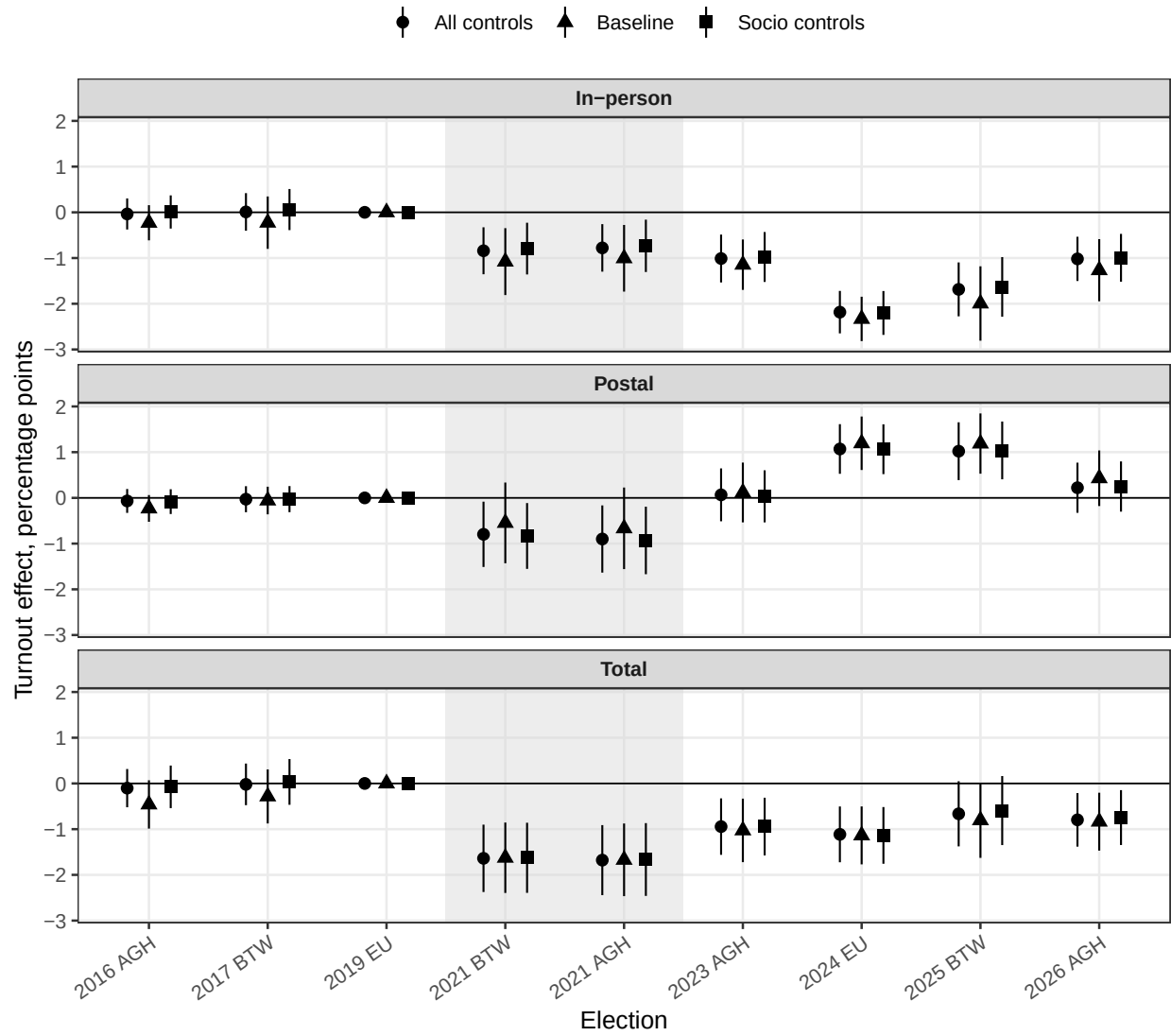
The resulting estimates are not interchangeable: sample restrictions change the population represented, and some election families contain different voting-eligibility rules. In-person turnout effects generally retain a negative sign across these comparisons, whereas net total-turnout effects are less precisely estimated in some reduced samples. The 2026 extension requires special caution because both the state franchise and the source-to-target geographic allocation differ from earlier years.

Table C1: Persistence within election types: in-person, postal, and total turnout

Election family	Election	(1) In-person turnout	(2) Postal turnout	(3) Total turnout
State elections (ref.: 2016 AGH)	2021 AGH	-0.777* (0.307)	-0.435 (0.477)	-1.211** (0.395)
State elections (ref.: 2016 AGH)	2023 AGH	-0.917*** (0.259)	0.349 (0.345)	-0.568 (0.353)
State elections (ref.: 2016 AGH)	2026 AGH	-1.037*** (0.286)	0.661+ (0.348)	-0.377 (0.311)
Federal elections (ref.: 2017 federal)	2021 federal	-0.852*** (0.221)	-0.490 (0.460)	-1.342** (0.430)
Federal elections (ref.: 2017 federal)	2025 federal	-1.768*** (0.269)	1.246*** (0.367)	-0.522 (0.371)
European elections (ref.: 2019 European)	2024 European	-2.332*** (0.247)	1.195*** (0.298)	-1.137*** (0.323)
<i>Fixed-effects</i>				
Precinct		Yes	Yes	Yes
Election $\times$ District		Yes	Yes	Yes
<i>Fit statistics</i>				
State elections observations		5,400	5,400	5,400
Federal elections observations		4,050	4,050	4,050
European elections observations		2,700	2,700	2,700

Notes: The table re-estimates the event-study specification within election families. These checks address the concern that the pooled panel combines federal, state, and European elections with different baseline turnout levels. State elections use 2016 AGH as their reference; 2026 has a newly eligible 16+ electorate. The table is descriptive of sensitivity to election-family comparisons, not a direct test of effects being equal across specifications.

Figure C1: Controls robustness: event-study estimates



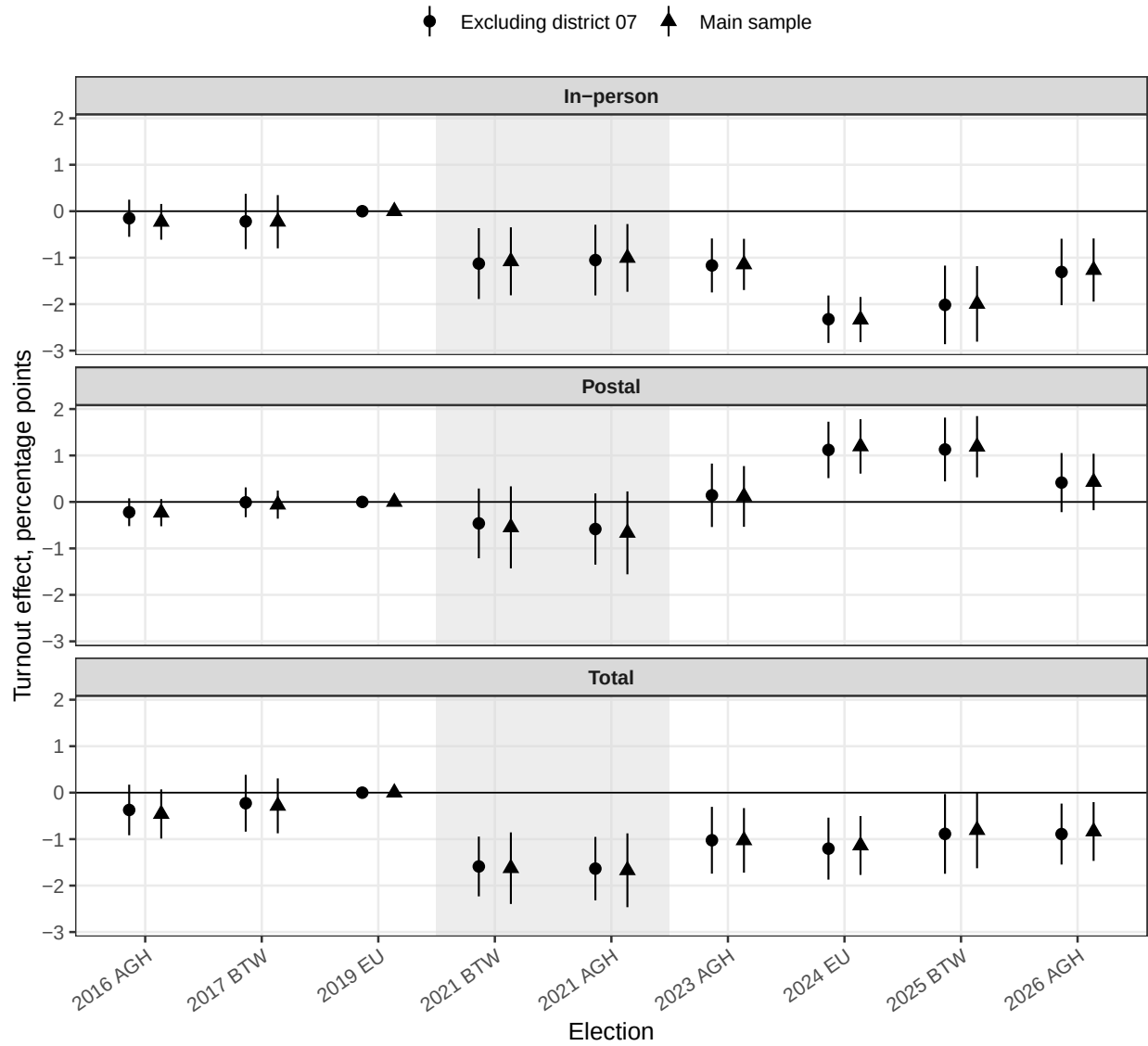
Notes: The figure compares event-study coefficients across the baseline specification and models with additional controls. Estimates are shown for in-person, postal, and total turnout. The reference election is the 2019 European election. The companion table is [Table C2](#).

Table C2: Controls robustness

<i>Panel A: In-person turnout</i>			
Election	(1) baseline	(2) socio controls	(3) all controls
2016 AGH	-0.228 (0.196)	0.008 (0.185)	-0.036 (0.173)
2017 federal	-0.226 (0.292)	0.060 (0.229)	0.009 (0.210)
2021 federal	-1.078** (0.373)	-0.793** (0.288)	-0.841** (0.262)
2021 AGH	-1.005** (0.372)	-0.733* (0.292)	-0.778** (0.265)
2023 AGH	-1.145*** (0.281)	-0.977*** (0.278)	-1.011*** (0.267)
2024 European	-2.332*** (0.247)	-2.201*** (0.245)	-2.184*** (0.237)
2025 federal	-1.994*** (0.415)	-1.632*** (0.333)	-1.686*** (0.301)
2026 AGH	-1.266*** (0.346)	-0.995*** (0.267)	-1.018*** (0.247)
<i>Panel B: Postal turnout</i>			
Election	(1) baseline	(2) socio controls	(3) all controls
2016 AGH	-0.231 (0.149)	-0.082 (0.138)	-0.067 (0.133)
2017 federal	-0.058 (0.154)	-0.027 (0.146)	-0.029 (0.145)
2021 federal	-0.547 (0.450)	-0.833* (0.367)	-0.797* (0.364)
2021 AGH	-0.666 (0.455)	-0.931* (0.377)	-0.899* (0.375)
2023 AGH	0.118 (0.333)	0.034 (0.290)	0.066 (0.295)
2024 European	1.195*** (0.298)	1.064*** (0.278)	1.070*** (0.277)
2025 federal	1.188*** (0.336)	1.038** (0.322)	1.021** (0.322)
2026 AGH	0.430 (0.310)	0.248 (0.280)	0.222 (0.280)
<i>Panel C: Total turnout</i>			
Election	(1) baseline	(2) socio controls	(3) all controls
2016 AGH	-0.459+ (0.270)	-0.074 (0.237)	-0.103 (0.213)
2017 federal	-0.284 (0.301)	0.033 (0.255)	-0.020 (0.232)
2021 federal	-1.625*** (0.393)	-1.626*** (0.391)	-1.638*** (0.376)
2021 AGH	-1.671*** (0.405)	-1.664*** (0.406)	-1.678*** (0.391)
2023 AGH	-1.027** (0.354)	-0.943** (0.322)	-0.944** (0.315)
2024 European	-1.137*** (0.323)	-1.137*** (0.316)	-1.114*** (0.311)
2025 federal	-0.806+ (0.419)	-0.593 (0.385)	-0.664+ (0.363)
2026 AGH	-0.836** (0.323)	-0.746* (0.306)	-0.796** (0.299)
<i>Fixed-effects</i>			
Precinct	Yes	Yes	Yes
Election × District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	12,150	12,150	12,150

Notes: The table reports the main event-study specification with additional pre-treatment socio-demographic and turnout controls. The reference election is the 2019 European election. The corresponding figure is [Figure C1](#).

Figure C2: Event-study estimates excluding district 07



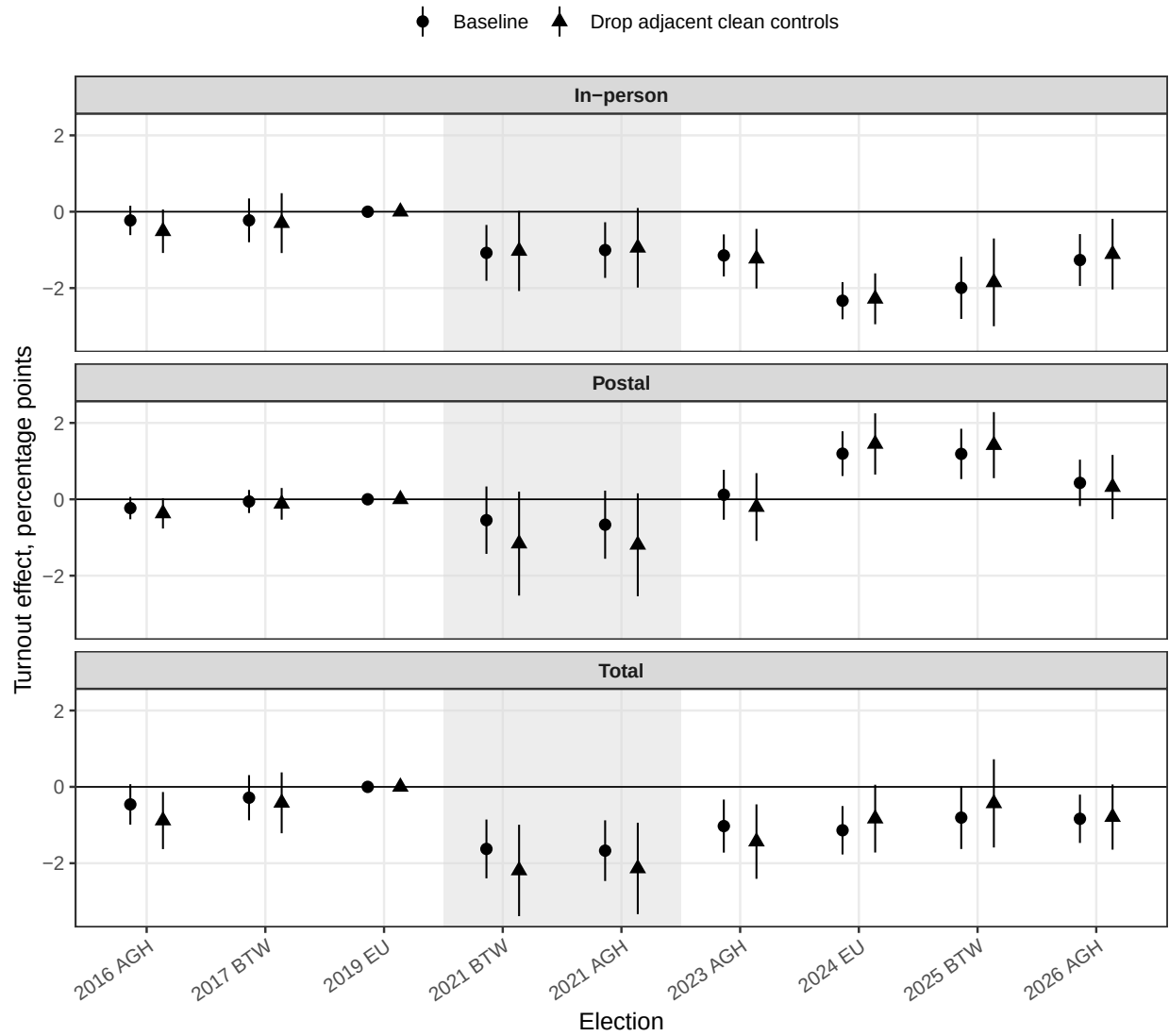
Notes: The figure plots the main event-study estimates after excluding district 07. This check addresses the fact that allocation to 2021 postal precincts was not unambiguous for the 2021 election in district 07. Outcomes are turnout levels in percentage points. The companion table is [Table C3](#).

Table C3: Robustness excluding district 07

Election	(1) In-person turnout	(2) Postal turnout	(3) Total turnout
2016 AGH	-0.151 (0.204)	-0.221 (0.152)	-0.373 (0.278)
2017 federal	-0.220 (0.303)	-0.008 (0.163)	-0.227 (0.312)
2021 federal	-1.127** (0.389)	-0.462 (0.383)	-1.589*** (0.328)
2021 AGH	-1.052** (0.387)	-0.583 (0.391)	-1.635*** (0.348)
2023 AGH	-1.167*** (0.296)	0.143 (0.348)	-1.024** (0.366)
2024 European	-2.324*** (0.260)	1.119*** (0.310)	-1.205*** (0.340)
2025 federal	-2.016*** (0.431)	1.129** (0.350)	-0.887* (0.437)
2026 AGH	-1.307*** (0.364)	0.416 (0.324)	-0.891** (0.334)
<i>Fixed-effects</i>			
Precinct	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	11,034	11,034	11,034

Notes: The table replicates the main event-study specification after excluding district 07, where allocation to 2021 postal precincts was not unambiguous for the 2021 election. The corresponding figure is [Figure C2](#).

Figure C3: Adjacent-control spillover check



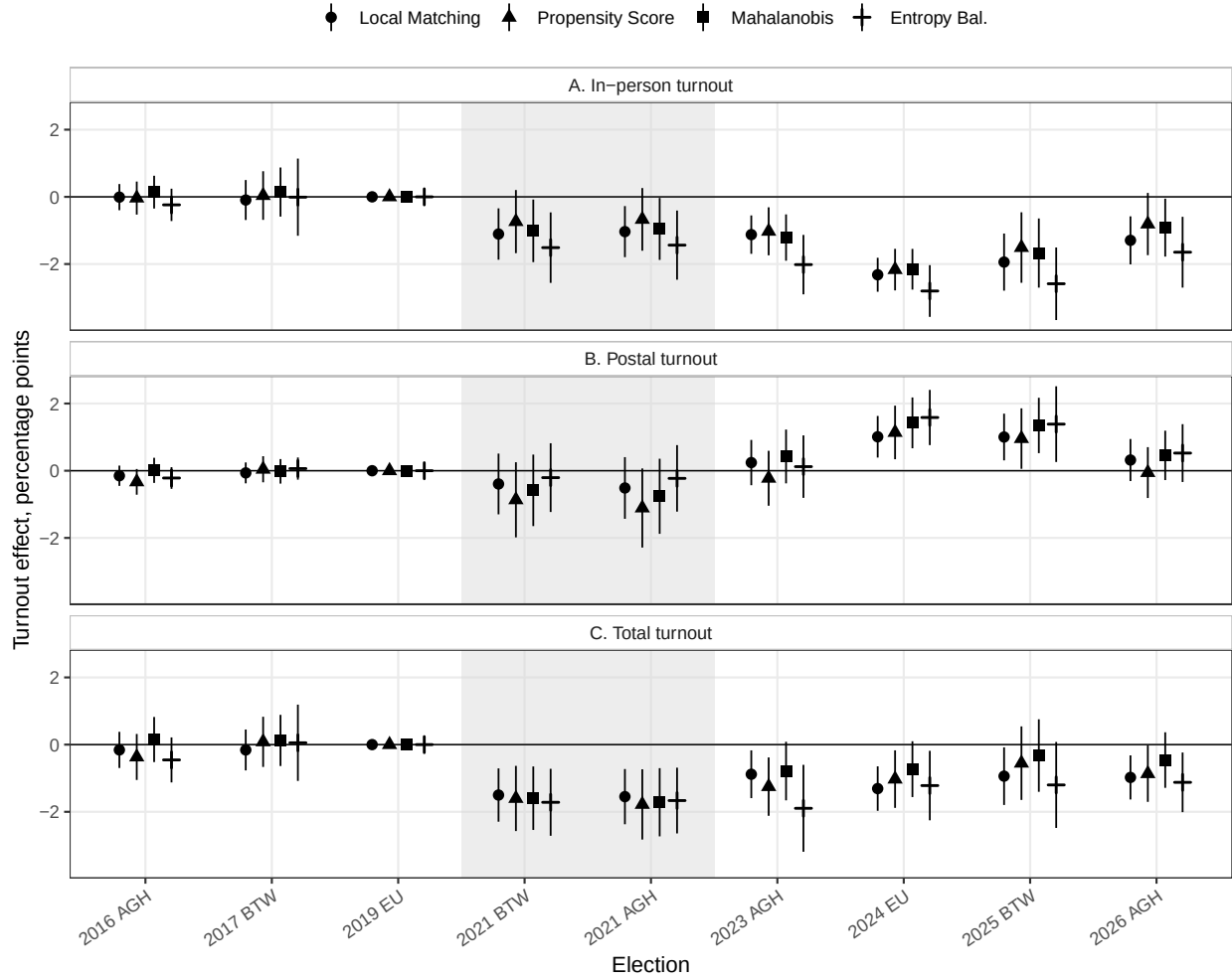
Notes: The figure compares the baseline estimates to estimates dropping clean-control precincts adjacent to treated precincts. This is a conservative local-spillover diagnostic. The reference election is the 2019 European election. The companion table is [Table C4](#).

Table C4: Adjacent-control spillover check

<i>Panel A: In-person turnout</i>		
	(1)	(2)
Election	Baseline	Drop adjacent controls
2016 AGH	-0.228 (0.196)	-0.512 ⁺ (0.290)
2017 federal	-0.226 (0.292)	-0.299 (0.399)
2021 federal	-1.078** (0.373)	-1.028 ⁺ (0.535)
2021 AGH	-1.005** (0.372)	-0.944 ⁺ (0.531)
2023 AGH	-1.145*** (0.281)	-1.230** (0.398)
2024 European	-2.332*** (0.247)	-2.281*** (0.339)
2025 federal	-1.994*** (0.415)	-1.850** (0.586)
2026 AGH	-1.266*** (0.346)	-1.112* (0.471)
<i>Panel B: Postal turnout</i>		
	(1)	(2)
Election	Baseline	Drop adjacent controls
2016 AGH	-0.231 (0.149)	-0.371 ⁺ (0.201)
2017 federal	-0.058 (0.154)	-0.119 (0.211)
2021 federal	-0.547 (0.450)	-1.161 ⁺ (0.693)
2021 AGH	-0.666 (0.455)	-1.192 ⁺ (0.687)
2023 AGH	0.118 (0.333)	-0.203 (0.452)
2024 European	1.195*** (0.298)	1.449*** (0.408)
2025 federal	1.188*** (0.336)	1.418** (0.441)
2026 AGH	0.430 (0.310)	0.321 (0.428)
<i>Panel C: Total turnout</i>		
	(1)	(2)
Election	Baseline	Drop adjacent controls
2016 AGH	-0.459 ⁺ (0.270)	-0.882* (0.381)
2017 federal	-0.284 (0.301)	-0.418 (0.406)
2021 federal	-1.625*** (0.393)	-2.189*** (0.610)
2021 AGH	-1.671*** (0.405)	-2.136*** (0.610)
2023 AGH	-1.027** (0.354)	-1.433** (0.497)
2024 European	-1.137*** (0.323)	-0.832 ⁺ (0.452)
2025 federal	-0.806 ⁺ (0.419)	-0.432 (0.589)
2026 AGH	-0.836** (0.323)	-0.791 ⁺ (0.435)
<i>Fixed-effects</i>		
Precinct	Yes	Yes
Election × District	Yes	Yes
<i>Fit statistics</i>		
Observations	12,150	8,055

Notes: The table drops clean-control precincts adjacent to treated precincts. The reference election is the 2019 European election. The corresponding figure is [Figure C3](#).

Figure C4: Matching robustness by turnout mode



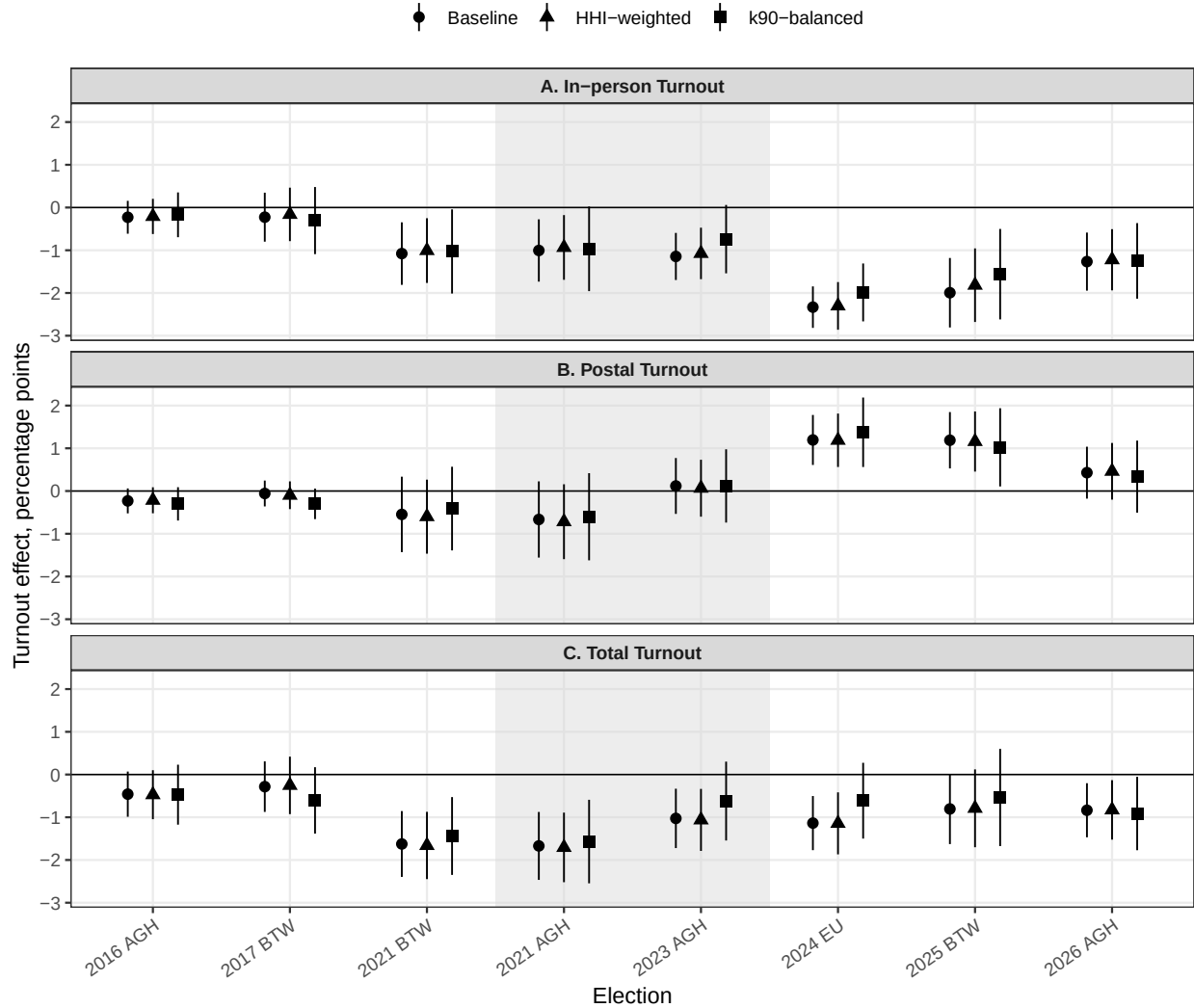
Notes: The figure reports event-study estimates for in-person, postal, and total turnout under four matching specifications: local matching, propensity-score matching, Mahalanobis matching, and entropy balancing. The reference election is the 2019 European election. The companion table is [Table C5](#).

Table C5: Matching robustness by turnout mode

<i>Panel A: In-person turnout</i>				
Election	(1) Local Matching	(2) Propensity Score	(3) Mahalanobis	(4) Entropy Bal.
2016 AGH	-0.008 (0.198)	-0.037 (0.250)	0.140 (0.248)	-0.239 (0.245)
2017 federal	-0.094 (0.303)	0.039 (0.368)	0.143 (0.372)	-0.010 (0.585)
2021 federal	-1.107** (0.389)	-0.737 (0.478)	-1.012* (0.474)	-1.512** (0.535)
2021 AGH	-1.035** (0.387)	-0.671 (0.474)	-0.955* (0.469)	-1.438** (0.525)
2023 AGH	-1.124*** (0.291)	-1.026** (0.364)	-1.212*** (0.351)	-2.017*** (0.451)
2024 European	-2.320*** (0.257)	-2.166*** (0.315)	-2.155*** (0.308)	-2.803*** (0.392)
2025 federal	-1.942*** (0.433)	-1.509** (0.534)	-1.673** (0.524)	-2.587*** (0.551)
2026 AGH	-1.295*** (0.364)	-0.810+ (0.471)	-0.916* (0.439)	-1.648** (0.537)
<i>Panel B: Postal turnout</i>				
Election	(1) Local Matching	(2) Propensity Score	(3) Mahalanobis	(4) Entropy Bal.
2016 AGH	-0.149 (0.154)	-0.331+ (0.195)	0.011 (0.190)	-0.217 (0.164)
2017 federal	-0.065 (0.158)	0.044 (0.197)	-0.018 (0.186)	0.064 (0.168)
2021 federal	-0.394 (0.461)	-0.866 (0.569)	-0.584 (0.541)	-0.205 (0.522)
2021 AGH	-0.513 (0.467)	-1.109+ (0.599)	-0.760 (0.569)	-0.228 (0.504)
2023 AGH	0.242 (0.343)	-0.224 (0.416)	0.425 (0.407)	0.122 (0.474)
2024 European	1.011** (0.315)	1.139** (0.406)	1.425*** (0.385)	1.584*** (0.419)
2025 federal	1.003** (0.354)	0.957* (0.458)	1.347** (0.419)	1.387* (0.574)
2026 AGH	0.318 (0.319)	-0.055 (0.385)	0.457 (0.374)	0.525 (0.439)
<i>Panel C: Total turnout</i>				
Election	(1) Local Matching	(2) Propensity Score	(3) Mahalanobis	(4) Entropy Bal.
2016 AGH	-0.158 (0.274)	-0.368 (0.348)	0.152 (0.341)	-0.455 (0.341)
2017 federal	-0.159 (0.310)	0.082 (0.380)	0.125 (0.387)	0.055 (0.579)
2021 federal	-1.501*** (0.404)	-1.603** (0.493)	-1.595*** (0.481)	-1.717*** (0.508)
2021 AGH	-1.549*** (0.419)	-1.780*** (0.532)	-1.716*** (0.517)	-1.665*** (0.499)
2023 AGH	-0.882* (0.362)	-1.250** (0.442)	-0.787+ (0.443)	-1.895** (0.659)
2024 European	-1.309*** (0.337)	-1.027* (0.436)	-0.731+ (0.423)	-1.219* (0.527)
2025 federal	-0.939* (0.437)	-0.552 (0.557)	-0.325 (0.549)	-1.200+ (0.652)
2026 AGH	-0.977** (0.334)	-0.865* (0.427)	-0.460 (0.421)	-1.123* (0.453)
<i>Fixed-effects</i>				
Precinct	Yes	Yes	Yes	Yes
Election × District	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	6,561	4,086	4,104	12,150

Notes: The table reports matching-based robustness checks for in-person, postal, and total turnout. Columns vary by matching specification. The reference election is the 2019 European election. The corresponding figure is [Figure C4](#).

Figure C5: Harmonization robustness: HHI weighting and k90-balanced sample



Notes: Historical precinct boundaries are mapped to the 2021 postal-precinct geography with population-weighted crosswalks. The baseline specification uses the full harmonized panel. The HHI-weighted specification assigns more weight to observations whose source-to-target mapping is more concentrated. The k90-balanced specification restricts the sample to harmonized units for which a small number of source units accounts for most of the 2021 target mass. The reference election is the 2019 European election. The companion table is [Table C6](#).

Table C6: Harmonization robustness: HHI weighting and k90-balanced sample

<i>Panel A: In-person turnout</i>			
Election	(1) baseline	(2) HHI-weighted	(3) k90-balanced
2016 AGH	-0.228 (0.196)	-0.209 (0.210)	-0.171 (0.267)
2017 federal	-0.226 (0.292)	-0.161 (0.319)	-0.305 (0.400)
2021 federal	-1.078** (0.373)	-1.007** (0.386)	-1.028* (0.501)
2021 AGH	-1.005** (0.372)	-0.934* (0.386)	-0.966+ (0.504)
2023 AGH	-1.145*** (0.281)	-1.075*** (0.307)	-0.740+ (0.408)
2024 European	-2.332*** (0.247)	-2.302*** (0.284)	-1.987*** (0.345)
2025 federal	-1.994*** (0.415)	-1.818*** (0.439)	-1.560** (0.539)
2026 AGH	-1.266*** (0.346)	-1.222*** (0.365)	-1.249** (0.450)
<i>Panel B: Postal turnout</i>			
Election	(1) baseline	(2) HHI-weighted	(3) k90-balanced
2016 AGH	-0.231 (0.149)	-0.216 (0.155)	-0.301 (0.199)
2017 federal	-0.058 (0.154)	-0.099 (0.165)	-0.300+ (0.182)
2021 federal	-0.547 (0.450)	-0.600 (0.442)	-0.411 (0.499)
2021 AGH	-0.666 (0.455)	-0.718 (0.447)	-0.602 (0.519)
2023 AGH	0.118 (0.333)	0.066 (0.339)	0.119 (0.437)
2024 European	1.195*** (0.298)	1.188*** (0.318)	1.375*** (0.414)
2025 federal	1.188*** (0.336)	1.159** (0.359)	1.023* (0.466)
2026 AGH	0.430 (0.310)	0.462 (0.338)	0.337 (0.429)
<i>Panel C: Total turnout</i>			
Election	(1) baseline	(2) HHI-weighted	(3) k90-balanced
2016 AGH	-0.459+ (0.270)	-0.472 (0.292)	-0.471 (0.357)
2017 federal	-0.284 (0.301)	-0.254 (0.343)	-0.606 (0.396)
2021 federal	-1.625*** (0.393)	-1.661*** (0.401)	-1.439** (0.464)
2021 AGH	-1.671*** (0.405)	-1.706*** (0.415)	-1.569** (0.498)
2023 AGH	-1.027** (0.354)	-1.062** (0.370)	-0.621 (0.470)
2024 European	-1.137*** (0.323)	-1.143** (0.370)	-0.612 (0.452)
2025 federal	-0.806+ (0.419)	-0.791+ (0.465)	-0.537 (0.579)
2026 AGH	-0.836** (0.323)	-0.828* (0.355)	-0.912* (0.437)
<i>Fixed-effects</i>			
Precinct	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	12,150	12,150	6,345

Notes: Baseline columns use the full population-weighted panel. HHI-weighted columns weight observations by crosswalk concentration. k90-balanced columns restrict the sample to harmonized units with relatively simple crosswalks. The reference election is the 2019 European election. The corresponding figure is [Figure C5](#).

Table C7: Auxiliary polling-station-precinct analysis for in-person turnout

Election	(1) In-person turnout
2016 AGH	-0.262 (0.182)
2017 federal	-0.318 (0.256)
2021 federal	-1.211*** (0.337)
2021 AGH	-1.117*** (0.334)
2023 AGH	-1.254*** (0.253)
2024 European	-2.430*** (0.233)
2025 federal	-2.267*** (0.378)
2026 AGH	-1.439*** (0.319)
<i>Fixed-effects</i>	
Polling-station precinct	Yes
Election $\times$ District	Yes
<i>Fit statistics</i>	
Observations	18,862

Notes: The table reports in-person turnout models at the polling-station-precinct geography. The analysis is auxiliary because postal and total turnout are analyzed at the postal-precinct geography in the main panel. The corresponding geographic comparison appears in main-text Figure 3.

Table C8: Turnout-above-100 exclusion sensitivity: 2026 state-election estimates

Outcome	Full analysis sample	Excluding excess-turnout rows
In-person turnout	-1.266 (0.346)	-1.266 (0.346)
Postal turnout	+0.430 (0.310)	+0.430 (0.310)
Total turnout	-0.836 (0.323)	-0.836 (0.323)

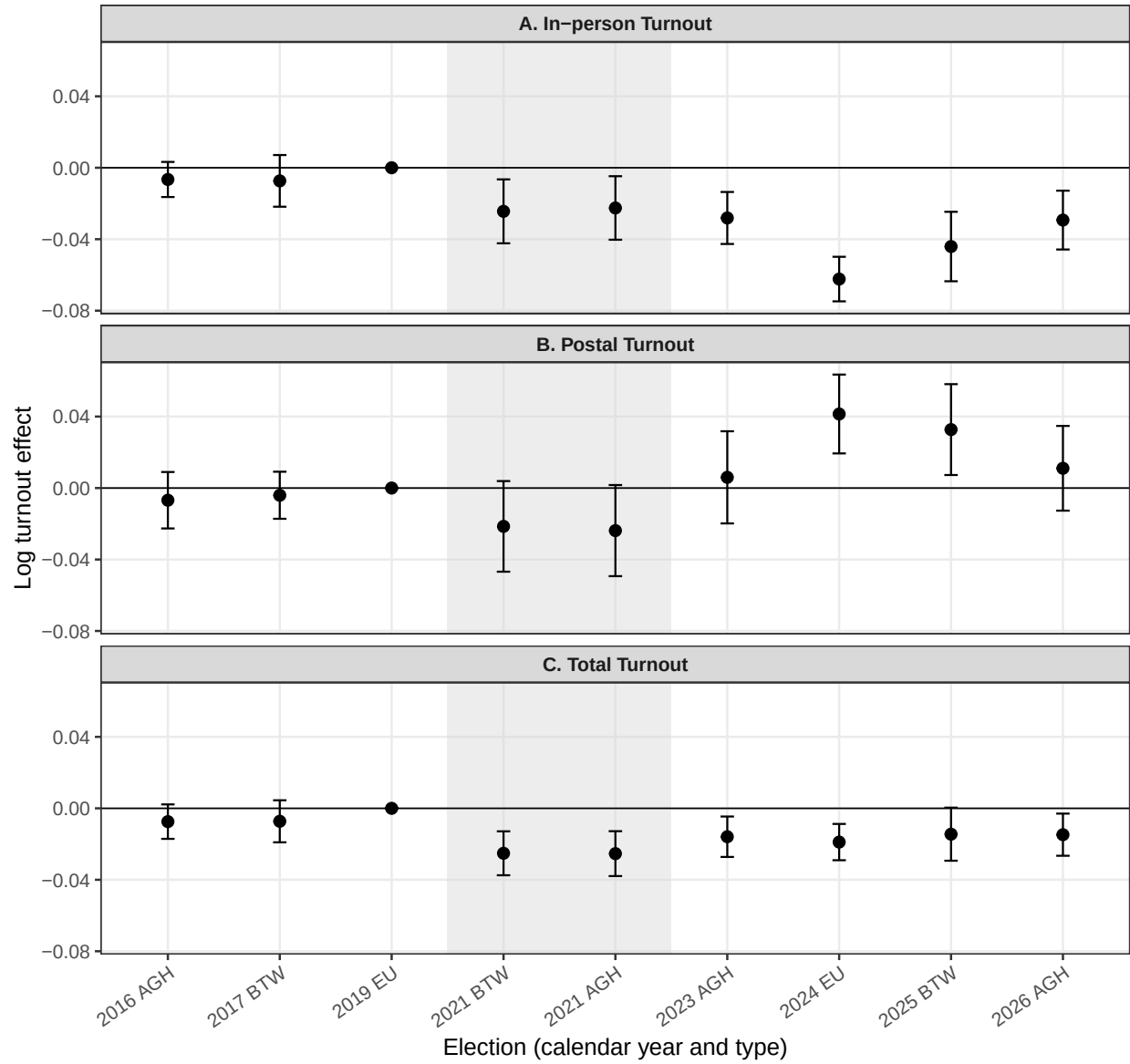
Notes: Entries are event-study coefficients in percentage points, with precinct-clustered standard errors in parentheses and the 2019 European election as reference. The restricted sample removes all 18 precinct-election observations whose interpolated total turnout exceeds 100 percent (17 in the two 2021 returns and one in the 2025 federal return; 16 lie in district 07). The reported comparison concerns the terminal 2026 contrast and does not establish an unchanged full event-study path. The R output also saves event-by-event estimates for verification after re-estimation.

Table C9: Rerun and election-omission checks

<i>Panel A: In-person turnout</i>				
Election	(1) Excluding AH23	(2) Excluding EU24	(3) Post-rerun only	(4) Before federal rerun
2016 AGH	-0.228 (0.196)	-0.228 (0.196)		-0.228 (0.196)
2017 federal	-0.226 (0.292)	-0.226 (0.292)		-0.226 (0.292)
2021 federal	-1.078** (0.373)	-1.078** (0.373)		-1.078** (0.373)
2021 AGH	-1.005** (0.372)	-1.005** (0.372)		-1.005** (0.372)
2023 AGH		-1.145*** (0.281)		-1.145*** (0.281)
2024 European	-2.332*** (0.247)		-1.186*** (0.184)	
2025 federal	-1.994*** (0.415)	-1.994*** (0.415)	-0.849** (0.290)	
2026 AGH	-1.266*** (0.346)	-1.266*** (0.346)	-0.120 (0.269)	
<i>Panel B: Postal turnout</i>				
Election	(1) Excluding AH23	(2) Excluding EU24	(3) Post-rerun only	(4) Before federal rerun
2016 AGH	-0.231 (0.149)	-0.231 (0.149)		-0.231 (0.149)
2017 federal	-0.058 (0.154)	-0.058 (0.154)		-0.058 (0.154)
2021 federal	-0.547 (0.450)	-0.547 (0.450)		-0.547 (0.450)
2021 AGH	-0.666 (0.455)	-0.666 (0.455)		-0.666 (0.455)
2023 AGH		0.118 (0.333)		0.118 (0.333)
2024 European	1.195*** (0.298)		1.077*** (0.303)	
2025 federal	1.188*** (0.336)	1.188*** (0.336)	1.070** (0.334)	
2026 AGH	0.430 (0.310)	0.430 (0.310)	0.311 (0.256)	
<i>Panel C: Total turnout</i>				
Election	(1) Excluding AH23	(2) Excluding EU24	(3) Post-rerun only	(4) Before federal rerun
2016 AGH	-0.459+ (0.270)	-0.459+ (0.270)		-0.459+ (0.270)
2017 federal	-0.284 (0.301)	-0.284 (0.301)		-0.284 (0.301)
2021 federal	-1.625*** (0.393)	-1.625*** (0.393)		-1.625*** (0.393)
2021 AGH	-1.671*** (0.405)	-1.671*** (0.405)		-1.671*** (0.405)
2023 AGH		-1.027** (0.354)		-1.027** (0.354)
2024 European	-1.137*** (0.323)		-0.110 (0.328)	
2025 federal	-0.806+ (0.419)	-0.806+ (0.419)	0.221 (0.424)	
2026 AGH	-0.836** (0.323)	-0.836** (0.323)	0.191 (0.329)	
<i>Fixed-effects</i>				
Precinct	Yes	Yes	Yes	Yes
Election × District	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	10,800	10,800	5,400	8,100

Notes: The table reports specifications that omit or re-reference elections around the 2023 state rerun and the 2024 partial federal rerun. The 2024 partial federal rerun is not included as a main panel outcome because it occurred only in affected areas.

Figure C6: Log-turnout sensitivity



Notes: The figure replicates the main event-study specification using logged turnout rates. The preferred specification in the main text uses turnout levels in percentage points because the substantive estimand is an absolute participation change. The companion table is [Table C10](#).

Table C10: Log-turnout sensitivity

Election	(1) In-person turnout	(2) Postal turnout	(3) Total turnout
2016 AGH	-0.007 (0.005)	-0.007 (0.008)	-0.007 (0.005)
2017 federal	-0.007 (0.007)	-0.004 (0.007)	-0.007 (0.006)
2021 federal	-0.024** (0.009)	-0.021+ (0.013)	-0.025*** (0.006)
2021 AGH	-0.023* (0.009)	-0.024+ (0.013)	-0.025*** (0.006)
2023 AGH	-0.028*** (0.007)	0.006 (0.013)	-0.016** (0.006)
2024 European	-0.062*** (0.006)	0.041*** (0.011)	-0.019*** (0.005)
2025 federal	-0.044*** (0.010)	0.033* (0.013)	-0.014+ (0.008)
2026 AGH	-0.029*** (0.008)	0.011 (0.012)	-0.015* (0.006)
<i>Fixed-effects</i>			
Precinct	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	12,150	12,150	12,150

Notes: The table replicates the main event-study specification using logged turnout rates. The preferred specification uses turnout levels in percentage points because the substantive estimand is an absolute participation change. The corresponding figure is [Figure C6](#).

D Disruption families and severity

The federal court-linked indicator remains the primary treatment. State-record classifications allow supplementary comparisons between documented impediments to polling-place access and ballot-error-only records. Because separate regressions on different samples do not provide a direct test of between-family differences, the joint specifications estimate both families with identical clean controls, common fixed effects, and the covariance needed to test their contrasts.

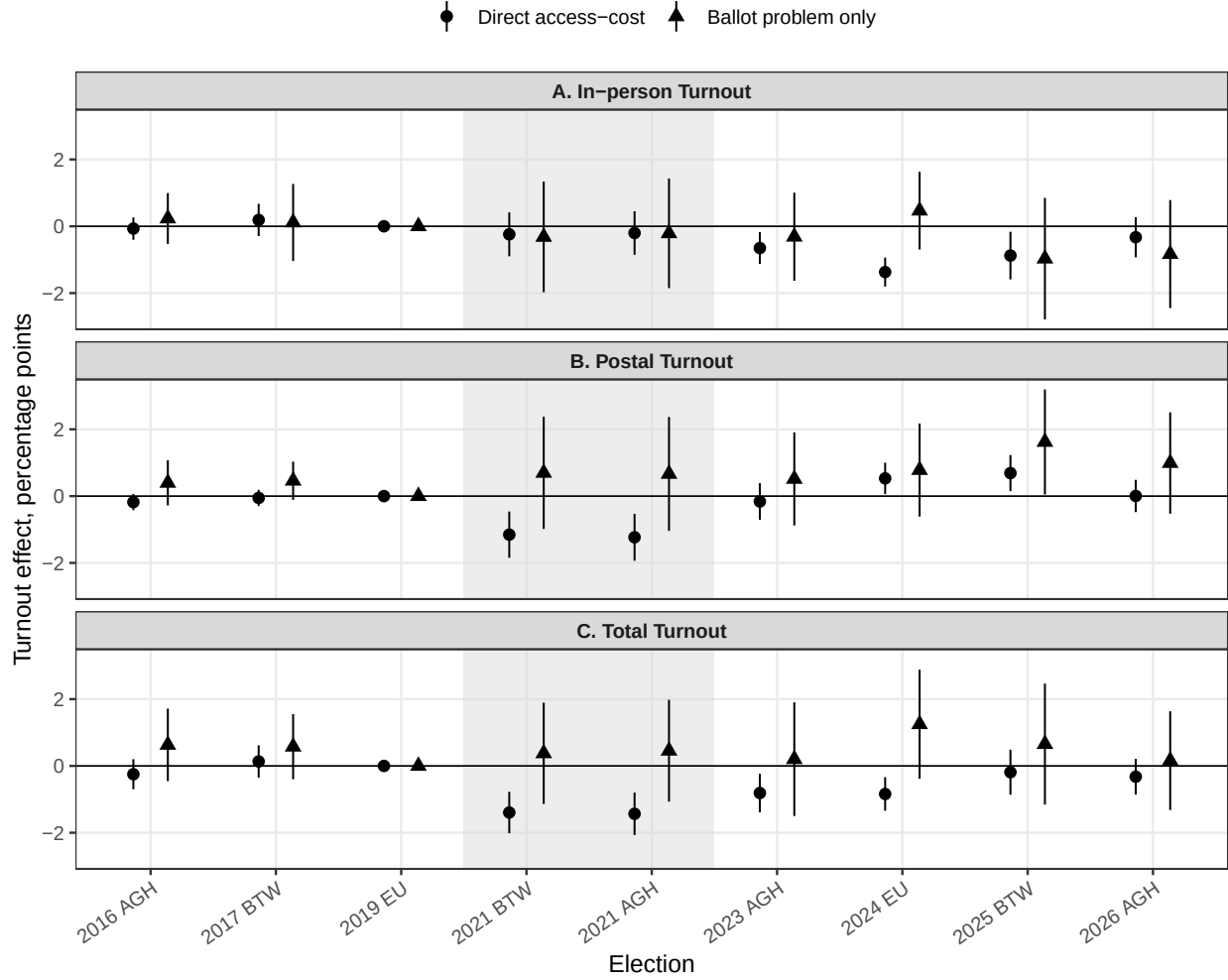
The postal result in 2021 is especially useful as a timing diagnostic because many postal ballots were cast before election-day disruptions occurred. Excluding district 07 addresses a documented concern about the allocation of referendum postal ballots, but does not establish that every 2021 voting return has the same problem. The waiting/closure dose specifications are exploratory: their pre-treatment coefficients are larger than those of the preferred binary-treatment design. These checks may inform interpretation of the type of disruption but do not isolate a unique behavioral channel.

Table D1: Disruption-family estimates

Election	(1) In-person: direct access-cost	(2) In-person: ballot problem only	(3) Postal: direct access-cost	(4) Postal: ballot problem only	(5) Total: direct access-cost	(6) Total: ballot problem only
2016 AGH	-0.070 (0.171)	0.233 (0.388)	-0.179 (0.123)	0.397 (0.344)	-0.249 (0.229)	0.630 (0.554)
2017 federal	0.189 (0.246)	0.115 (0.588)	-0.055 (0.123)	0.460 (0.292)	0.134 (0.247)	0.575 (0.497)
2021 federal	-0.241 (0.335)	-0.319 (0.844)	-1.154** (0.354)	0.695 (0.856)	-1.395*** (0.316)	0.377 (0.773)
2021 AGH	-0.201 (0.332)	-0.210 (0.837)	-1.232*** (0.358)	0.664 (0.869)	-1.433*** (0.324)	0.455 (0.777)
2023 AGH	-0.651** (0.243)	-0.313 (0.672)	-0.160 (0.279)	0.515 (0.710)	-0.811** (0.295)	0.202 (0.867)
2024 European	-1.370*** (0.221)	0.468 (0.593)	0.530* (0.240)	0.780 (0.709)	-0.840** (0.256)	1.248 (0.832)
2025 federal	-0.876* (0.364)	-0.968 (0.927)	0.688* (0.276)	1.622* (0.801)	-0.188 (0.344)	0.654 (0.923)
2026 AGH	-0.328 (0.306)	-0.832 (0.824)	0.005 (0.246)	0.990 (0.773)	-0.323 (0.273)	0.158 (0.753)
<i>Fixed-effects</i>						
Precinct	Yes	Yes	Yes	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	13,032	9,882	13,032	9,882	13,032	9,882

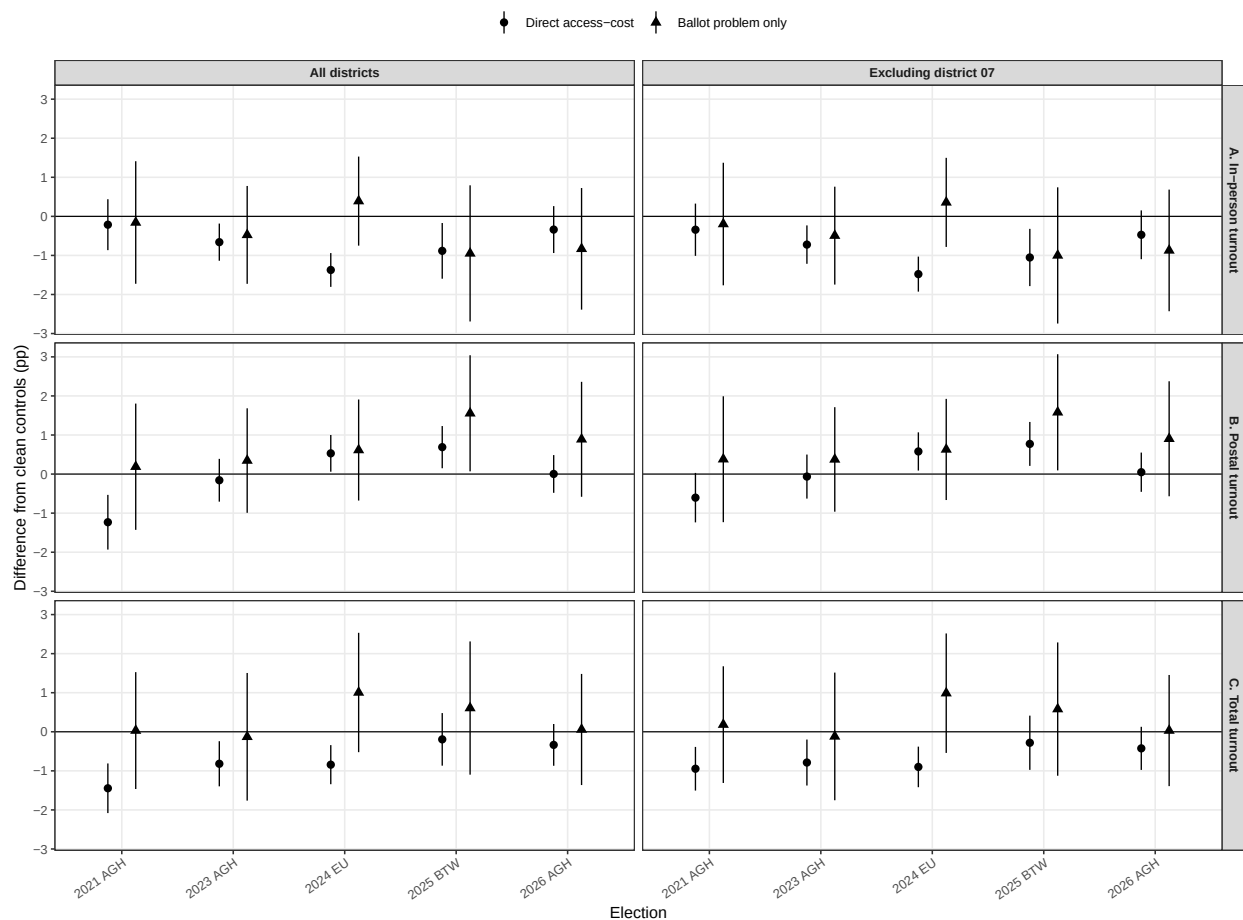
Notes: The table reports event-study estimates for two broad state-record disruption families. These estimates support interpretation of the mechanism and are not the preferred treatment definition. The corresponding figure is [Figure D1](#).

Figure D1: Voting-mode outcomes by documented disruption family



Notes: The figure displays separate state-record event-study models and does not by itself test whether their coefficients differ. Formal pooled contrasts, within-family estimates, and the district-07 exclusion are reported in [Figure D2](#) and [tables D2](#) and [D3](#). Direct access/cost disruptions include waiting, voting after closing, closure or interruption, and missing ballots. Ballot-problem-only records refer to ballot errors without a direct access/cost disruption. Estimates are event-study coefficients relative to the 2019 European reference election and are shown separately by turnout mode. The two 2021 outcomes are concurrent federal and state ballots from the same election day and should not be interpreted as independent exposure events. Vertical bars show 95 percent confidence intervals clustered by 2021 postal precinct. Companion regression estimates are reported in [Table D1](#).

Figure D2: Within-family turnout responses: all districts and excluding Tempelhof-Schöneberg



Notes: Separate event-study paths for the access-cost and ballot-only families are estimated in a single pooled model with a common clean-control group. The left panels include Berlin; the right panels exclude district 07. The reference is the 2019 European election; intervals are 95% cluster-robust confidence intervals. The 2021 postal estimates are contemporaneous diagnostics, not evidence of switching caused by election-day disruption. The accompanying numerical estimates and pooled contrasts are reported below.

Table D2: Within-family event-study estimates, with and without district 07

Sample	Outcome	Election	Direct access (SE)	Ballot only (SE)
All districts	In-person	2021 AGH	-0.213 (0.333)	-0.157 (0.800)
All districts	In-person	2023 AGH	-0.660 (0.243)	-0.474 (0.639)
All districts	In-person	2024 EU	-1.372 (0.221)	0.390 (0.582)
All districts	In-person	2025 BTW	-0.883 (0.364)	-0.948 (0.889)
All districts	In-person	2026 AGH	-0.338 (0.306)	-0.831 (0.794)
All districts	Postal	2021 AGH	-1.232 (0.357)	0.188 (0.825)
All districts	Postal	2023 AGH	-0.158 (0.279)	0.346 (0.682)
All districts	Postal	2024 EU	0.530 (0.240)	0.615 (0.660)
All districts	Postal	2025 BTW	0.689 (0.276)	1.556 (0.758)
All districts	Postal	2026 AGH	0.003 (0.246)	0.890 (0.751)
All districts	Total	2021 AGH	-1.446 (0.324)	0.031 (0.763)
All districts	Total	2023 AGH	-0.819 (0.295)	-0.128 (0.833)
All districts	Total	2024 EU	-0.842 (0.255)	1.006 (0.779)
All districts	Total	2025 BTW	-0.194 (0.343)	0.608 (0.870)
All districts	Total	2026 AGH	-0.335 (0.273)	0.059 (0.726)
Excluding district 07	In-person	2021 AGH	-0.342 (0.341)	-0.197 (0.800)
Excluding district 07	In-person	2023 AGH	-0.723 (0.251)	-0.493 (0.639)
Excluding district 07	In-person	2024 EU	-1.479 (0.229)	0.358 (0.582)
Excluding district 07	In-person	2025 BTW	-1.053 (0.374)	-1.000 (0.889)
Excluding district 07	In-person	2026 AGH	-0.472 (0.319)	-0.872 (0.794)
Excluding district 07	Postal	2021 AGH	-0.604 (0.323)	0.380 (0.822)
Excluding district 07	Postal	2023 AGH	-0.064 (0.288)	0.375 (0.682)
Excluding district 07	Postal	2024 EU	0.579 (0.249)	0.630 (0.660)
Excluding district 07	Postal	2025 BTW	0.772 (0.287)	1.581 (0.759)
Excluding district 07	Postal	2026 AGH	0.048 (0.256)	0.904 (0.751)
Excluding district 07	Total	2021 AGH	-0.946 (0.285)	0.183 (0.762)
Excluding district 07	Total	2023 AGH	-0.787 (0.300)	-0.119 (0.834)
Excluding district 07	Total	2024 EU	-0.900 (0.265)	0.988 (0.780)
Excluding district 07	Total	2025 BTW	-0.281 (0.354)	0.581 (0.871)
Excluding district 07	Total	2026 AGH	-0.424 (0.282)	0.031 (0.726)

Notes: Event-by-family estimates are compared with the same clean-control group. This is a distinct sensitivity from the preferred federal-court treatment; the contrast across disruption families is tested separately in [Table D3](#).

Table D3: Differences between disruption families: pooled comparisons, with and without district 07

Sample	Outcome	Election	Access minus ballot (pp)	SE of difference	p-value
All districts	In-person	2021 AGH	-0.056	0.827	0.946
All districts	In-person	2023 AGH	-0.186	0.658	0.777
All districts	In-person	2024 EU	-1.763	0.604	0.004
All districts	In-person	2025 BTW	0.065	0.916	0.943
All districts	In-person	2026 AGH	0.493	0.817	0.546
All districts	In-person	Joint 2023–26	–	–	0.000
All districts	Postal	2021 AGH	-1.421	0.854	0.096
All districts	Postal	2023 AGH	-0.504	0.707	0.476
All districts	Postal	2024 EU	-0.085	0.666	0.898
All districts	Postal	2025 BTW	-0.867	0.778	0.265
All districts	Postal	2026 AGH	-0.887	0.766	0.247
All districts	Postal	Joint 2023–26	–	–	0.083
All districts	Total	2021 AGH	-1.477	0.802	0.066
All districts	Total	2023 AGH	-0.690	0.863	0.424
All districts	Total	2024 EU	-1.848	0.788	0.019
All districts	Total	2025 BTW	-0.802	0.887	0.366
All districts	Total	2026 AGH	-0.394	0.746	0.597
All districts	Total	Joint 2023–26	–	–	0.138
Excluding district 07	In-person	2021 AGH	-0.146	0.828	0.860
Excluding district 07	In-person	2023 AGH	-0.229	0.659	0.728
Excluding district 07	In-person	2024 EU	-1.836	0.606	0.002
Excluding district 07	In-person	2025 BTW	-0.053	0.917	0.954
Excluding district 07	In-person	2026 AGH	0.400	0.819	0.625
Excluding district 07	In-person	Joint 2023–26	–	–	0.000
Excluding district 07	Postal	2021 AGH	-0.984	0.845	0.244
Excluding district 07	Postal	2023 AGH	-0.439	0.709	0.536
Excluding district 07	Postal	2024 EU	-0.051	0.667	0.939
Excluding district 07	Postal	2025 BTW	-0.809	0.779	0.299
Excluding district 07	Postal	2026 AGH	-0.856	0.768	0.265
Excluding district 07	Postal	Joint 2023–26	–	–	0.101
Excluding district 07	Total	2021 AGH	-1.129	0.795	0.156
Excluding district 07	Total	2023 AGH	-0.668	0.865	0.440
Excluding district 07	Total	2024 EU	-1.888	0.789	0.017
Excluding district 07	Total	2025 BTW	-0.862	0.888	0.332
Excluding district 07	Total	2026 AGH	-0.456	0.747	0.542
Excluding district 07	Total	Joint 2023–26	–	–	0.124

Notes: Each outcome is fitted with both family-by-election interactions on one common sample with identical clean controls, 2021 postal-precinct and election-by-district fixed effects, and 2021 postal-precinct clustered covariance. Reported contrasts are direct access-cost minus ballot-problem-only coefficients (percentage points). The joint test covers 2023–2026. District 07 is Tempelhof-Schöneberg. The 2021 postal estimates are a concurrent diagnostic rather than a causal substitution effect.

Table D4: Dose-response models: baseline and waiting/closure severity

Election	(1) In-person: federal exposure	(2) In-person: waiting/closure severity	(3) Postal: federal exposure	(4) Postal: waiting/closure severity	(5) Total: federal exposure	(6) Total: waiting/closure severity
2016 AGH	-0.228 (0.196)	-0.323 (0.204)	-0.231 (0.149)	-0.309* (0.157)	-0.459+ (0.270)	-0.632* (0.297)
2017 federal	-0.226 (0.292)	-0.823** (0.297)	-0.058 (0.154)	0.076 (0.142)	-0.284 (0.301)	-0.747* (0.310)
2021 federal	-1.078** (0.373)	-2.186*** (0.358)	-0.547 (0.450)	0.192 (0.382)	-1.625*** (0.393)	-1.994*** (0.337)
2021 AGH	-1.005** (0.372)	-2.139*** (0.349)	-0.666 (0.455)	0.081 (0.388)	-1.671*** (0.405)	-2.058*** (0.349)
2023 AGH	-1.145*** (0.281)	-1.900*** (0.308)	0.118 (0.333)	0.928** (0.341)	-1.027** (0.354)	-0.973* (0.377)
2024 European	-2.332*** (0.247)	-2.284*** (0.267)	1.195*** (0.298)	1.200*** (0.303)	-1.137*** (0.323)	-1.084** (0.344)
2025 federal	-1.994*** (0.415)	-2.524*** (0.425)	1.188*** (0.336)	0.955** (0.352)	-0.806+ (0.419)	-1.570*** (0.430)
2026 AGH	-1.266*** (0.346)	-2.013*** (0.329)	0.430 (0.310)	0.640* (0.312)	-0.836** (0.323)	-1.374*** (0.338)
<i>Fixed-effects</i>						
Precinct	Yes	Yes	Yes	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	12,150	11,997	12,150	11,997	12,150	11,997

Notes: Columns (1), (3), and (5) report the baseline binary treatment for in-person, postal, and total turnout; Columns (2), (4), and (6) replace the binary treatment with waiting/closure severity. The reference election is the 2019 European election. The waiting/closure sample is nested in the baseline federal comparison: clean controls are coded as zero and federal treated precincts without state-record waiting/closure information are excluded. Pretrend diagnostics are reported in [Table D5](#).

Table D5: Waiting/closure severity pretrend diagnostics

Election	(1) In-person: federal exposure	(2) In-person: waiting/closure severity	(3) Postal: federal exposure	(4) Postal: waiting/closure severity	(5) Total: federal exposure	(6) Total: waiting/closure severity
2016 AGH	-0.228 (0.196)	-0.323 (0.204)	-0.231 (0.149)	-0.309* (0.157)	-0.459+ (0.270)	-0.632* (0.297)
2017 federal	-0.226 (0.292)	-0.823** (0.297)	-0.058 (0.154)	0.076 (0.142)	-0.284 (0.301)	-0.747* (0.310)
<i>Fixed-effects</i>						
Precinct	Yes	Yes	Yes	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	12,150	11,997	12,150	11,997	12,150	11,997

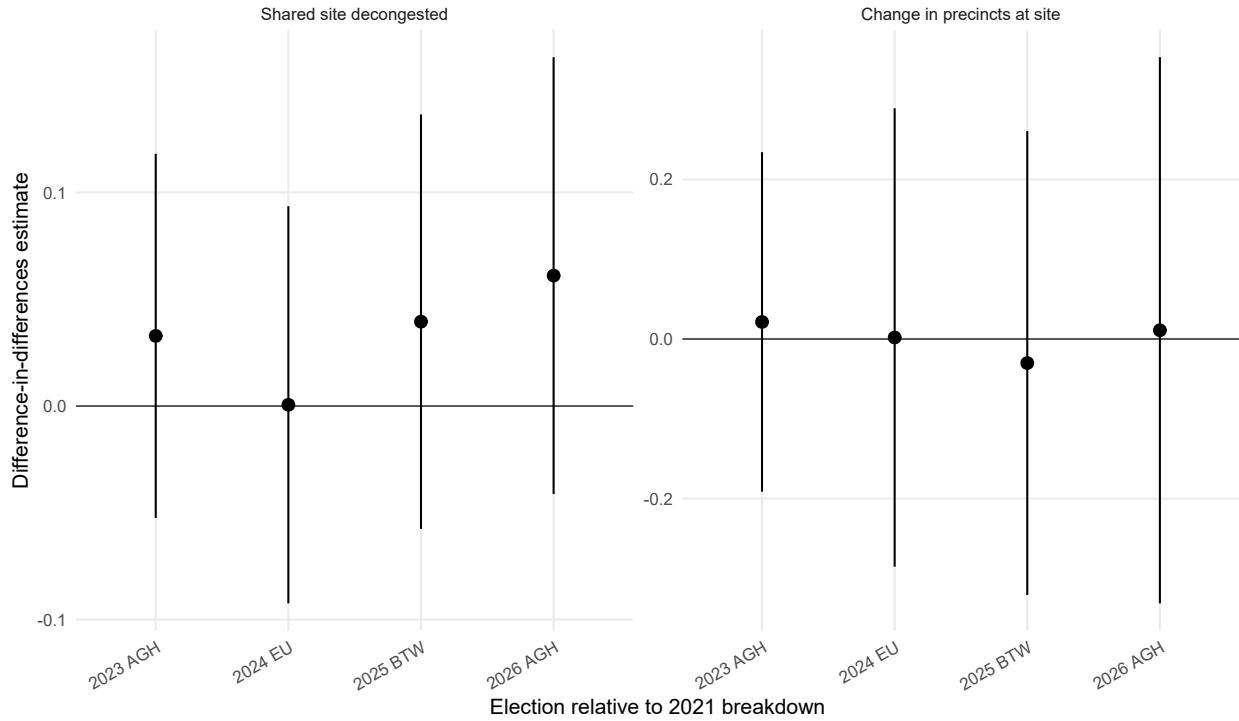
Notes: The table reports pre-2021 coefficients for the baseline federal treatment and the continuous waiting/closure-hours measure. The reference election is the 2019 European election. The continuous severity measure shows non-negligible pre-treatment differences and is therefore interpreted as exploratory mechanism evidence.

E Administrative adaptation and physical polling locations

The administrative-response checks use physical polling-location points rather than treating every UWB or postal-counting location as a distinct voter-facing site. Postal-counting facilities are removed before in-person point locations are grouped into physical sites using a 50-meter clustering criterion. The analytical sample follows 2021 sites that initially hosted at least two polling-station precincts. The outcomes describe whether original sites remain used, whether shared sites are decongested, the change in assigned precincts, and whether a different site appears within 500 meters.

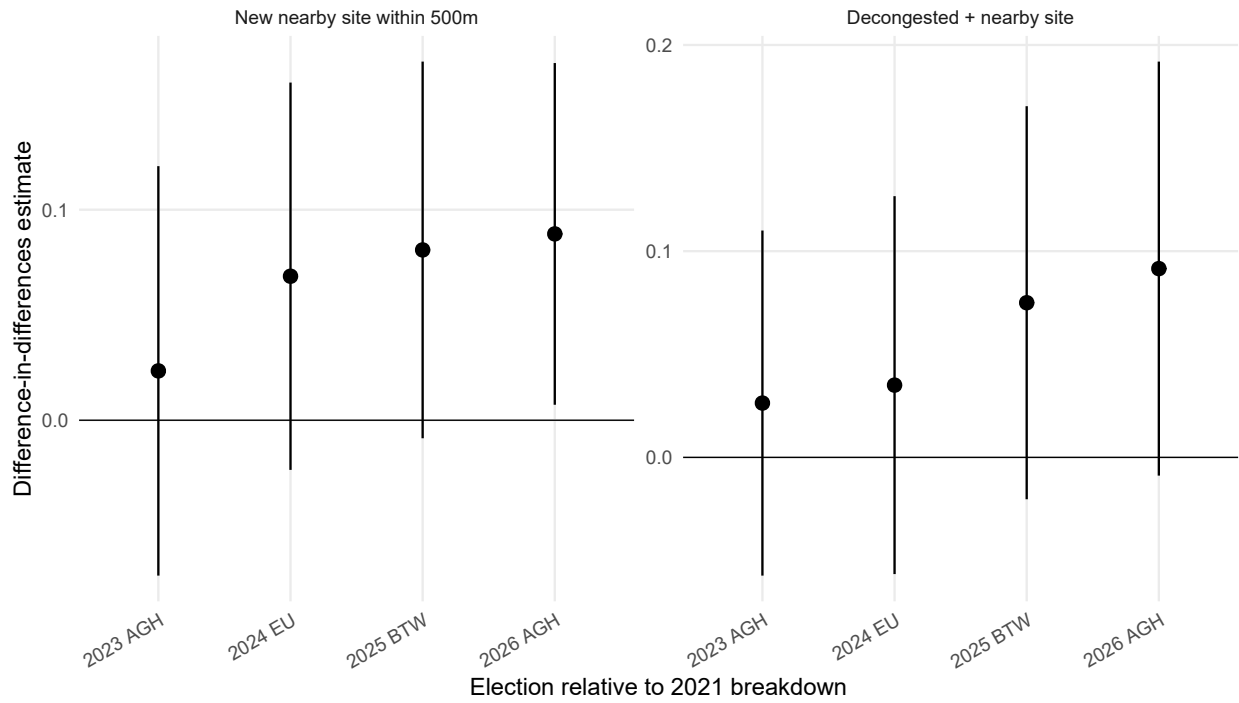
These measurements can detect differential changes in election administration after the breakdown. They are not a causal decomposition of the turnout effects: subsequent assignment decisions may respond to the earlier failure, to broader reorganization, or to other conditions affecting participation. The positive 2026 nearby-site estimate must be evaluated together with its uncertainty and the other administrative outcomes.

Figure E1: Administrative response: decongestion of shared 2021 polling locations



Notes: Unit is the 2021 physical polling location, defined by 50-meter clusters of in-person polling-location points after excluding `Briefwahl==1` postal-counting rows. The sample is restricted to locations that hosted at least two polling-station precincts in 2021. Estimates compare court-linked treated sites to clean-control sites, relative to AH21, with site fixed effects and election-by-district fixed effects. Numerical estimates for the administrative-response outcomes are reported in [Table E1](#).

Figure E2: Administrative response: nearby new polling sites



Notes: Unit is the 2021 physical polling location, defined by 50-meter clusters of in-person polling-location points after excluding `Briefwahl==1` postal-counting rows. A nearby site is a different later in-person polling-location cluster within 500 meters of the 2021 site. The sample is restricted to locations that hosted at least two polling-station precincts in 2021. Estimates compare court-linked treated sites to clean-control sites, relative to AH21. The analysis uses the same physical-site construction as [Figure E1](#). Numerical estimates are reported in [Table E1](#).

Table E1: Administrative-response estimates

Outcome	Election	Estimate	Std. error	95% CI	p-value
Shared site decongested	2023 AGH	0.033	0.043	[-0.052, 0.118]	0.449
Shared site decongested	2024 European	0.001	0.047	[-0.092, 0.094]	0.990
Shared site decongested	2025 federal	0.040	0.049	[-0.058, 0.137]	0.424
Shared site decongested	2026 AGH	0.061	0.052	[-0.041, 0.163]	0.241
Change in precincts at site	2023 AGH	0.021	0.108	[-0.191, 0.234]	0.844
Change in precincts at site	2024 European	0.002	0.146	[-0.285, 0.289]	0.990
Change in precincts at site	2025 federal	-0.030	0.148	[-0.321, 0.261]	0.839
Change in precincts at site	2026 AGH	0.011	0.174	[-0.331, 0.353]	0.950
Original site still used	2023 AGH	0.004	0.021	[-0.038, 0.045]	0.868
Original site still used	2024 European	-0.002	0.032	[-0.065, 0.061]	0.952
Original site still used	2025 federal	0.006	0.029	[-0.051, 0.064]	0.833
Original site still used	2026 AGH	0.040	0.034	[-0.027, 0.107]	0.246
New nearby site within 500m	2023 AGH	0.023	0.050	[-0.074, 0.121]	0.636
New nearby site within 500m	2024 European	0.068	0.047	[-0.024, 0.160]	0.145
New nearby site within 500m	2025 federal	0.081	0.046	[-0.009, 0.170]	0.076
New nearby site within 500m	2026 AGH	0.089	0.041	[0.007, 0.170]	0.033
Decongested + nearby site	2023 AGH	0.026	0.043	[-0.057, 0.110]	0.537
Decongested + nearby site	2024 European	0.035	0.047	[-0.057, 0.127]	0.453
Decongested + nearby site	2025 federal	0.075	0.049	[-0.020, 0.170]	0.123
Decongested + nearby site	2026 AGH	0.092	0.051	[-0.009, 0.192]	0.074

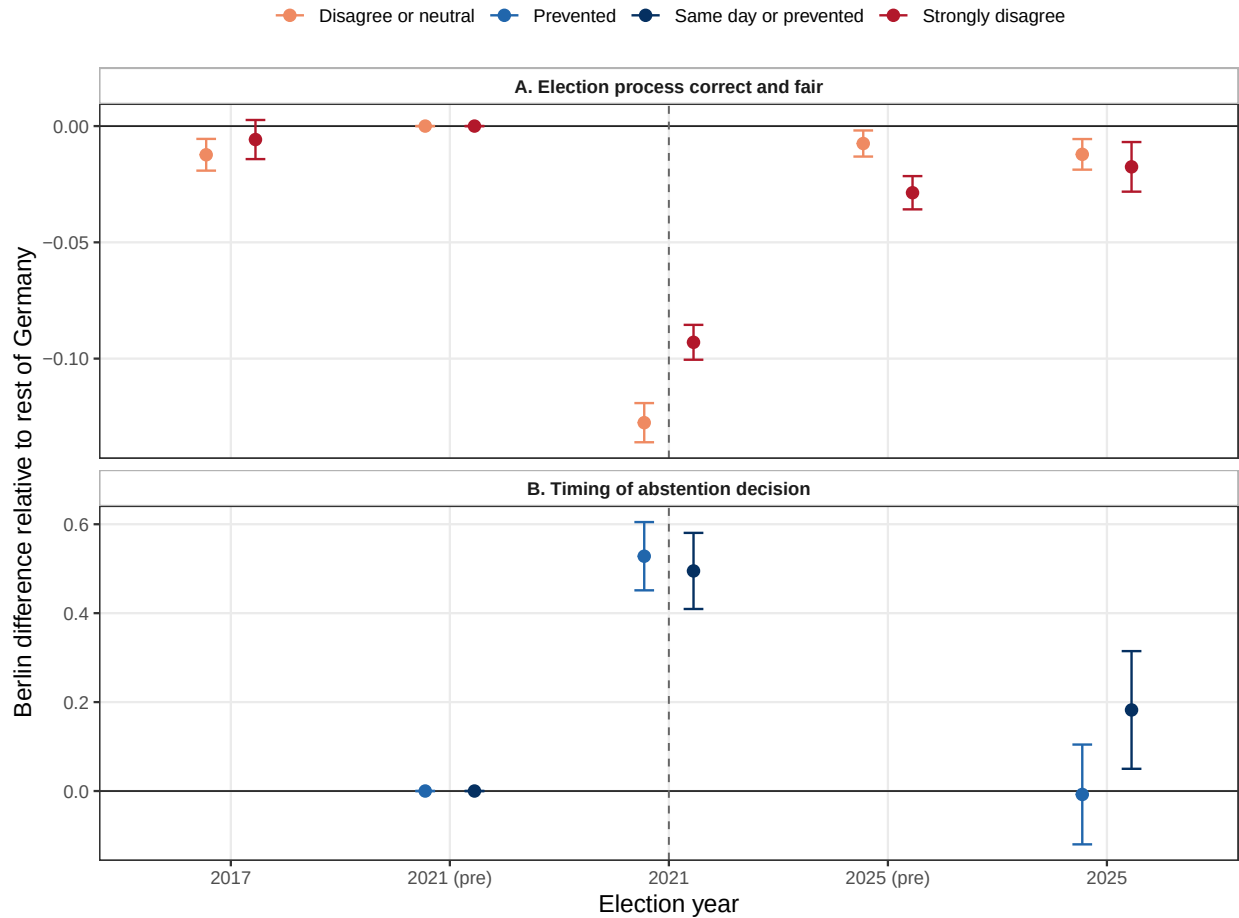
Notes: The table reports event-study estimates for the administrative-response outcomes shown in [Figures E1](#) and [E2](#). Unit is the 2021 physical polling location, defined by 50-meter clusters of in-person polling-location points. The sample is restricted to locations that hosted at least two polling-station precincts in 2021. Estimates compare court-linked treated sites to clean-control sites relative to AH21, with site fixed effects and election-by-district fixed effects.

F Survey context and exploratory outcomes

The German Longitudinal Election Study compares Berlin residents with respondents elsewhere in Germany on perceptions of election fairness and the timing of abstention decisions. Those comparisons concern a different geographic exposure than the precinct-based court designation. They can establish contextual salience but cannot attribute changes in beliefs to an individual’s encounter with a particular polling-place failure.

Party vote-share and neighborhood-heterogeneity estimates are exploratory extensions. Party vote shares condition on voters who cast ballots and may change with participation composition, while demographic interactions use local population characteristics rather than individual voter characteristics. Their inclusion here documents potential wider consequences without changing the participation outcomes or the primary identification claim.

Figure F1: GLES survey evidence on salience and perceived election administration



Notes: The figure reports Berlin-versus-rest-of-Germany differences in survey measures of election fairness, correctness, abstention timing, and reports of being prevented from voting. Because the survey lacks precinct-level exposure, these estimates are contextual salience evidence. Companion tables are [Tables F1](#) and [F2](#).

Table F1: GLES survey evidence: election fairness

Year	Strict fairness	Broad fairness	Continuous fairness
2017	-0.006 (0.004)	-0.012** (0.003)	-0.073** (0.021)
2021	-0.093*** (0.004)	-0.128*** (0.004)	-0.716*** (0.012)
2025 pre-election	-0.029*** (0.003)	-0.008* (0.003)	-0.123*** (0.010)
2025	-0.018** (0.005)	-0.012** (0.003)	-0.117*** (0.016)
<i>Fixed-effects</i>			
State	Yes	Yes	Yes
Year	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	20,213	31,484	31,484

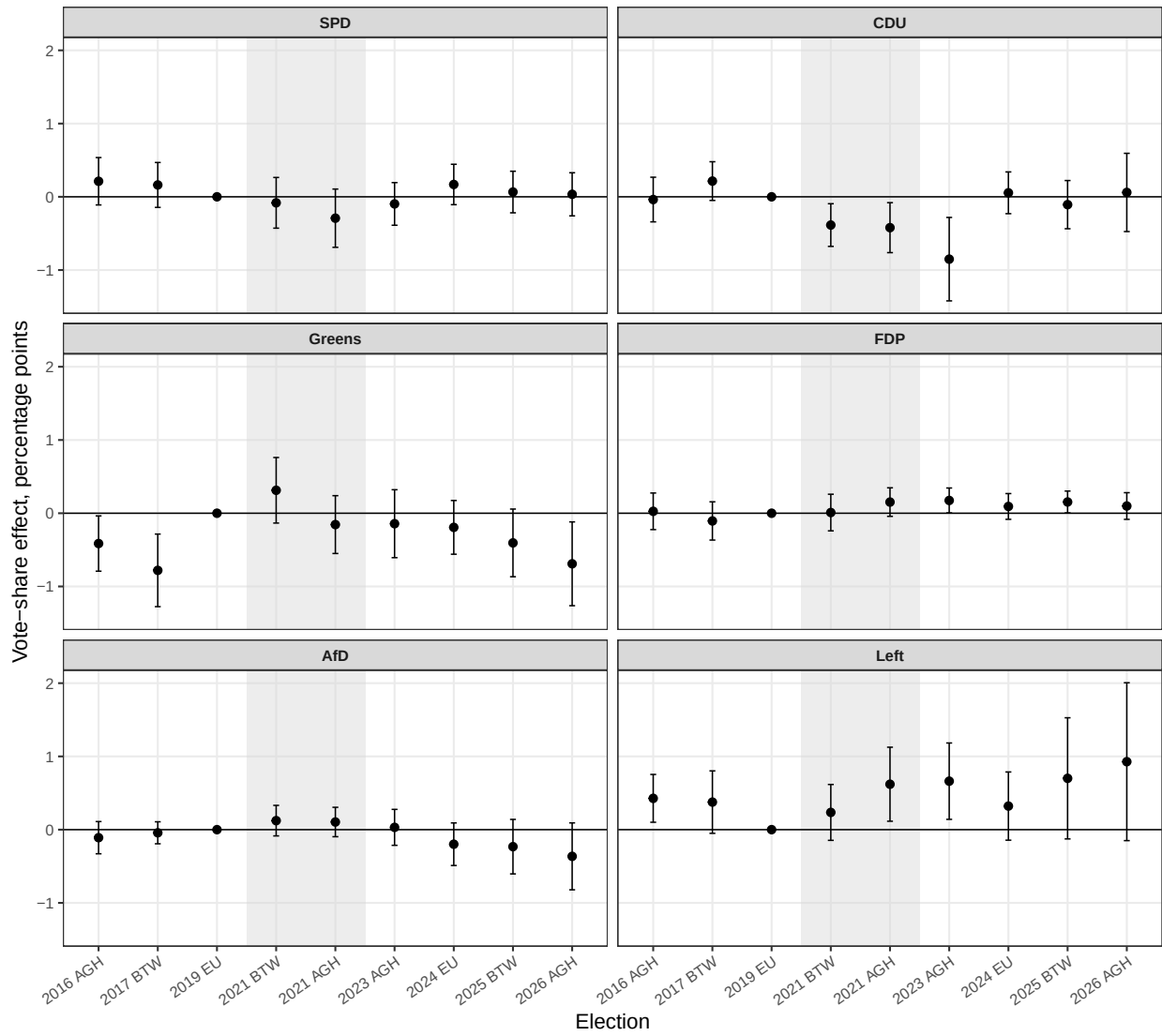
Notes: The table reports Berlin-versus-rest-of-Germany differences in survey measures of perceived election fairness and correctness using GLES data. The estimates are contextual mechanism evidence and do not identify which respondents personally experienced polling-place disruption.

Table F2: GLES survey evidence: abstention timing

Year	Prevented from voting	Same day/prevented
2017		
2021	0.528*** (0.036)	0.495*** (0.040)
2025 pre-election		
2025	-0.008 (0.053)	0.182* (0.062)
<i>Fixed-effects</i>		
State	Yes	Yes
Year	Yes	Yes
<i>Fit statistics</i>		
Observations	510	510

Notes: The table reports Berlin-versus-rest-of-Germany differences in abstention timing and reports of being prevented from voting. Because the survey lacks precinct-level exposure, the table should be interpreted as city-level evidence about experiences and perceptions rather than a direct individual-level treatment test.

Figure F2: Exploratory party vote-share estimates



Notes: The figure reports exploratory event-study estimates for party vote shares. Vote shares are conditional on votes cast and therefore reflect compositional changes among voters as well as any possible changes in party support. The companion table is [Table F3](#).

Table F3: Party vote-share estimates

Election	(1) SPD	(2) CDU	(3) Greens	(4) FDP	(5) AfD	(6) Left
2023 AGH	-0.097 (0.148)	-0.851** (0.290)	-0.143 (0.236)	0.174* (0.087)	0.031 (0.125)	0.662* (0.266)
2024 European	0.168 (0.141)	0.055 (0.145)	-0.193 (0.187)	0.093 (0.090)	-0.199 (0.148)	0.322 (0.237)
2025 federal	0.065 (0.145)	-0.107 (0.168)	-0.405+ (0.235)	0.154* (0.076)	-0.232 (0.190)	0.701+ (0.422)
2026 AGH	0.034 (0.150)	0.060 (0.272)	-0.690* (0.292)	0.098 (0.093)	-0.365 (0.233)	0.928+ (0.549)
<i>Fixed-effects</i>						
Precinct	Yes	Yes	Yes	Yes	Yes	Yes
Election $\times$ District	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	12,150	12,150	12,150	12,150	12,150	12,150

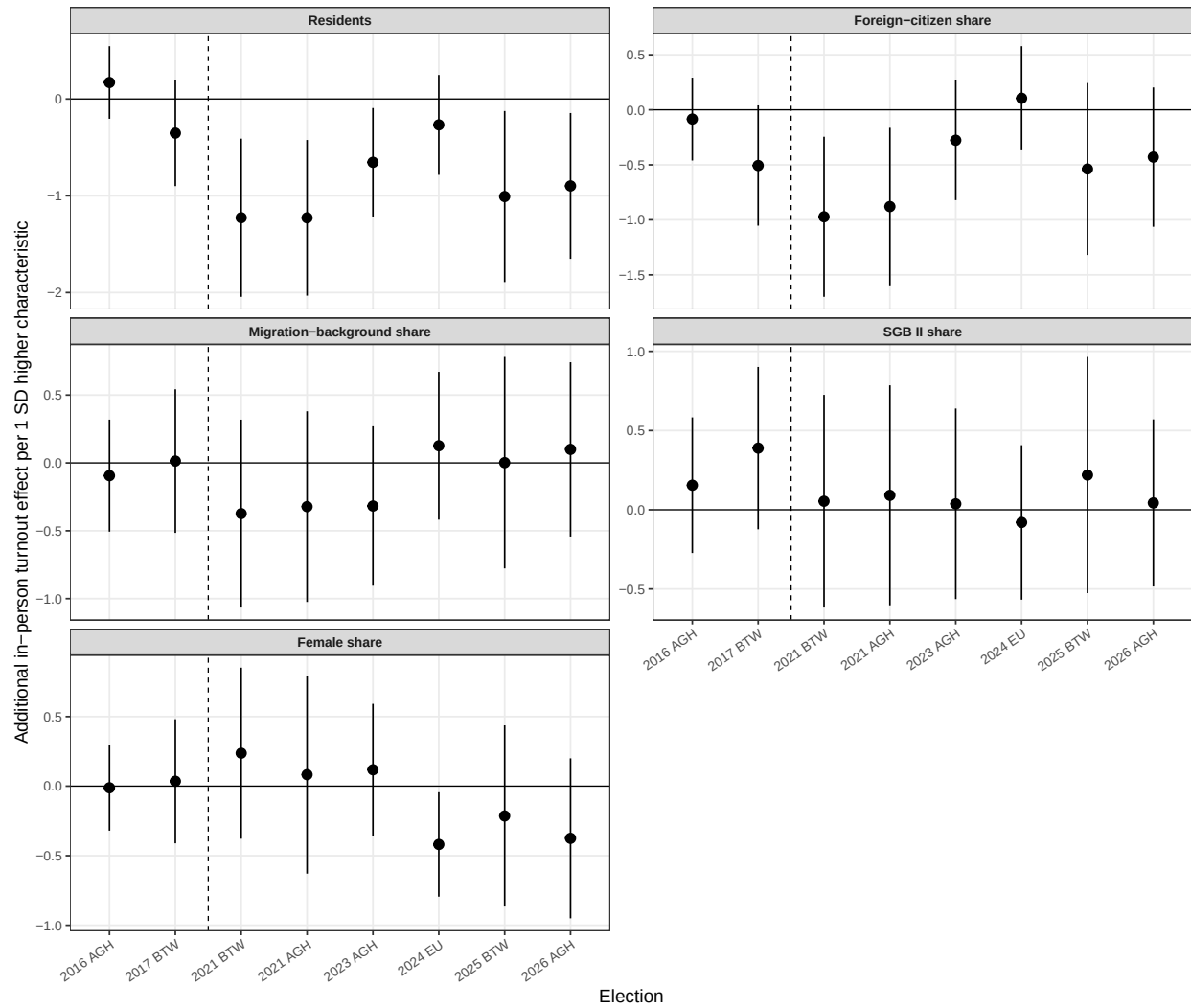
Notes: Exploratory party vote-share outcomes. Vote shares are conditional on turnout and therefore combine participation changes, mode substitution, and compositional changes among voters. The estimates are not interpreted as persuasion effects. The corresponding figure is [Figure F2](#).

Table F4: Socio-demographic heterogeneity estimates

Characteristic	2023 AGH	2024 European	2025 federal	2026 AGH
Residents	-0.654* (0.286)	-0.268 (0.263)	-1.008* (0.450)	-0.898* (0.384)
Foreign-citizen share	-0.277 (0.277)	0.105 (0.241)	-0.538 (0.398)	-0.430 (0.323)
Migration-background share	-0.318 (0.299)	0.126 (0.278)	0.002 (0.397)	0.100 (0.327)
SGB II share	0.037 (0.307)	-0.080 (0.249)	0.219 (0.380)	0.043 (0.269)
Female share	0.118 (0.242)	-0.419* (0.191)	-0.214 (0.332)	-0.375 (0.293)

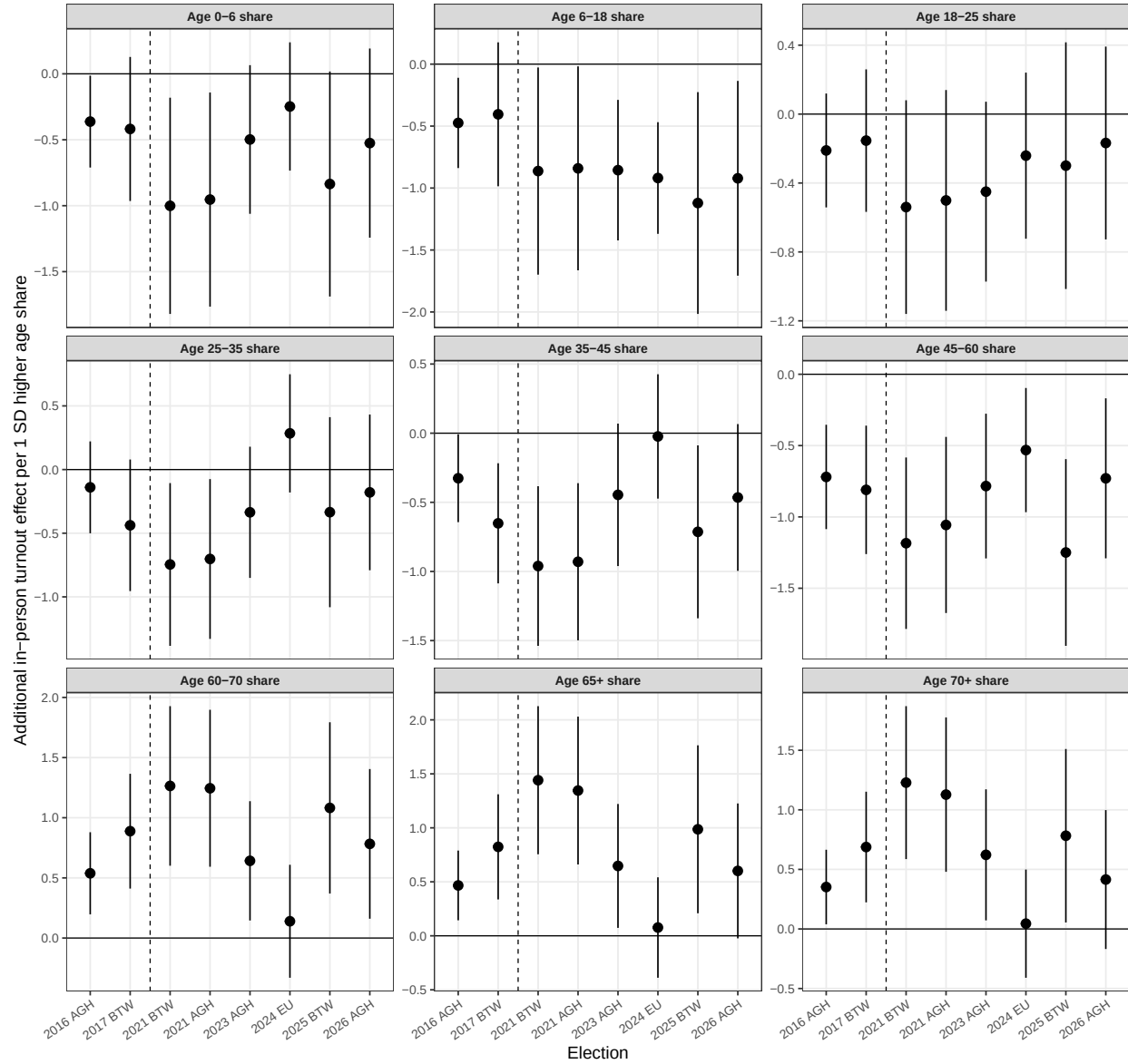
Notes: The table reports treatment-by-election interaction heterogeneity with continuous precinct characteristics standardized using their 2019 distribution. Coefficients describe the additional in-person turnout effect associated with one standard deviation higher pre-treatment characteristics; they are ecological and exploratory.

Figure F3: Socio-demographic heterogeneity diagnostics



Notes: The figure displays treatment-by-election interaction slopes per one-standard-deviation higher 2019 precinct characteristic. They are ecological exploratory estimates, not individual treatment effects. The companion table is [Table F4](#).

Figure F4: Age-composition heterogeneity diagnostics



Notes: The figure displays treatment-by-election slopes per one-standard-deviation increase in each 2019 precinct age share. These are exploratory ecological estimates, not effects among individuals of the named age. The companion table is [Table F5](#).

Table F5: Age-composition heterogeneity estimates

Age variable	2023 AGH	2024 European	2025 federal	2026 AGH
Age 0-6 share	-0.498 ⁺ (0.287)	-0.248 (0.248)	-0.835 ⁺ (0.435)	-0.525 (0.366)
Age 6-18 share	-0.856 ^{**} (0.289)	-0.919 ^{***} (0.230)	-1.121 [*] (0.456)	-0.921 [*] (0.401)
Age 18-25 share	-0.450 ⁺ (0.266)	-0.241 (0.246)	-0.299 (0.365)	-0.168 (0.285)
Age 25-35 share	-0.336 (0.263)	0.284 (0.237)	-0.335 (0.381)	-0.179 (0.312)
Age 35-45 share	-0.446 ⁺ (0.263)	-0.024 (0.229)	-0.713 [*] (0.319)	-0.465 ⁺ (0.271)
Age 45-60 share	-0.784 ^{**} (0.259)	-0.532 [*] (0.222)	-1.249 ^{***} (0.334)	-0.729 [*] (0.286)
Age 60-70 share	0.642 [*] (0.253)	0.139 (0.239)	1.082 ^{**} (0.363)	0.783 [*] (0.317)
Age 65+ share	0.647 [*] (0.292)	0.077 (0.237)	0.987 [*] (0.396)	0.601 ⁺ (0.318)
Age 70+ share	0.622 [*] (0.280)	0.045 (0.232)	0.782 [*] (0.371)	0.415 (0.297)

Notes: The table reports continuous treatment-by-election interaction heterogeneity using standardized 2019 precinct age composition. Coefficients refer to one-standard-deviation higher age shares, not separate individual age groups or median-split effects. These comparisons are ecological and exploratory.